Volume X. *March, 1900.* *Number 3.*

THE PHYSICAL REVIEW.

ON THE METABOLISM OF MATTER IN THE LIVING BODY.

By Edward B. Rosa.

A GREAT many so-called metabolism experiments have been made upon the human body and upon animals in which a balance of nitrogen or of nitrogen and carbon was obtained. That is, the quantity of nitrogen or of nitrogen and carbon in the food was determined by analyzing samples of the food, and the quantity of these elements in the excreta and the products of respiration was also determined. If the nitrogen taken into the body during the course of an experiment exceeds that given off from the body in the same period, then there is a net gain of nitrogen, and this is taken to indicate an increase in the total amount of protein of the body. Conversely, if the outgo of nitrogen exceeds the receipts there is a net loss, and this indicates a loss of protein. As protein contains 16 per cent. of nitrogen, the quantity of protein gained or lost is found by multiplying the gain or loss of nitrogen by 6.25. But protein also contains 53 per cent. of carbon. Hence, before striking a balance of the carbon, we must insert on one side or the other of the account, the quantity of carbon in the protein which has been gained or lost. The net gain or loss of carbon is then taken as a measure of the amount of fat which has been gained or lost from the body during the experiment. As fat contains 76.5 per cent. of carbon, the quantity of fat gained or lost is found by dividing the gain or loss of carbon by the factor .765.

These experiments in which both a nitrogen and carbon balance are determined require a determination, as has been said, not only of the nitrogen and carbon of the food and excreta, but also of the quantity of carbon dioxid given off from the lungs and skin; that is, of the products of respiration. They are hence called respiration experiments. Sometimes the subject of the experiment has worn a mask in such a manner as to respire air drawn from a particular source, the composition of which is determined, while the air expelled from the lungs is carried away to a reservoir, and its contents of oxygen and carbon dioxid subsequently determined. This gives the quantity of oxygen absorbed and of carbon dioxid exhaled. But such an experiment neglects the respiration of the skin, and is unsatisfactory on other accounts. The better method is to confine the subject in a chamber and to analyze samples of the air supplied to the chamber, and also other samples drawn from the current of air flowing away from the chamber.

In the experiment reported by Atwater and Rosa in the September and October numbers of the PHYSICAL REVIEW, as well as in subsequent experiments with the same apparatus, a nitrogen and carbon balance was obtained and also a balance of energy. For the latter, the potential energy of the food eaten by the subject was determined by burning samples in a calorimeter; the potential energy of the excreta was determined by burning samples of the dried residue in a similar manner; the heat given off from the body during the experiment was also measured, the respiration chamber being at the same time a calorimeter, hence called a respiration calorimeter. A balance of energy can then be reached by taking account of the quantity of protein and fat gained or lost. Such a balance of energy, to within about one or two per cent. has been obtained in a considerable number of experiments at Middletown, and the work is still being carried on.

A balance of hydrogen is more difficult to obtain than are the nitrogen and carbon balances. This involves, of course, the determination of the organic hydrogen of food and excreta, and also the quantity of water in the same and the quantity of water exhaled from the skin and lungs. The water vapor contained in the current of air entering the calorimetric chamber, and also that leaving through

the outgoing current of air are determined by absorption in sulphuric acid tubes at the same time that the carbon dioxid is absorbed by soda lime. But water being to some extent condensed upon the walls of the chamber, and absorbed by the clothing of the subject and the furnishings of the chamber, the exact determination of the water becomes a more difficult matter than that of the carbon dioxid, and a hydrogen balance has been seldom attempted.

It is impossible for one who has not been engaged in it to appreciate the difficulty and complexity of this kind of experimental work, or the enormous amount of labor and computation necessary to obtain the final results of a four days' respiration experiment. In the Middletown experiments analyses of samples of food were made in duplicate, and all determinations of carbon dioxid and water in the air current were in duplicate. But in spite of the greatest care residual errors are not inappreciable. And in weighing the food and drink, selecting samples for analysis, and in collecting and weighing the water condensed inside the chamber and outside in the freezers, errors of appreciable magnitude are bound to occur. Hence when one strikes a balance of nitrogen and carbon and computes the number of grams of protein and fat which have been gained or lost, the algebraic sum of a large number of errors appears in the result. Moreover, in an energy experiment this error is carried over into heat account in a magnified degree. Suppose, for example, that in the determination of the carbon of the food there is an error of one-half of one per cent.; the quantity of carbon in the daily ration being 261.5 g., this amounts to an error of 1.3 g., or 1.7 g. of fat. The heat value of a gram of fat being 9.4 large calories, 1.7 g. is equivalent to 16.0 calories, which is 0.7 per cent. of the total heat per day. An error in the nitrogen, however, produces a comparatively small error in the computed heat; for if the protein gained be reckoned too large, the fat gained will be too small in a corresponding degree, and the difference of heat value produced is relatively small.

In order to locate the sources of error and to reduce them in magnitude to a minimum, it is desirable to make use of every possible check upon the analyses and computations. Such a check may be found in the change of weight of the subject of the experi-

ment, provided the oxygen taken up from the air by respiration can be determined. Direct determinations of oxygen have been made in short experiments, by analyzing the air in the chamber at the beginning and at the end, and admitting to the chamber during the experiment a known quantity of oxygen. But for long experiments this is impracticable, as the air of the chamber must be frequently renewed. On the other hand, it is impossible to analyze the air by sample, as is done for CO_2 and H_2O, and determine the percentage of oxygen in the ingoing and outcoming air currents with sufficient accuracy. For if 50 liters of air be admitted to the chamber per minute, the quantity of oxygen in 24 hours will be about 20,000 g. and an error of one-tenth of one per cent. in the determination would amount to 20 grams, which would be an error of three per cent. in the amount of oxygen absorbed by the lungs.

On the other hand, an oxygen balance may be obtained in a manner similar to that followed for N, C and H. If the quantity of these latter elements and the ash of the food and excreta are accurately determined, then the amount of oxygen becomes known. The oxygen of the water of income and outgo is accurately known when the quantity of water is determined, and hence one can readily calculate the total amount of oxygen taken into the body and the total given out. The difference will represent that derived from the atmosphere by respiration. If it be objected that this determination of oxygen by difference is not sufficiently exact it may be replied, first, that if the oxygen is not exact then the other elements, N, C and H, are not exact; and, second, that the dry residue of food and excreta contains over 30 per cent. of oxygen, a greater proportion than that of any other constituent except carbon, and hence a determination by difference is far more reliable than where that difference is a small quantity, as, for example, in the case of the protein and fat gained or lost. But the latter quantities are regularly calculated by difference, with no check upon the results to show them to be correct. It is now proposed in a similar manner to calculate the hydrogen and oxygen by difference, and then to determine the oxygen taken up from the air in the manner of Pettenkofer and Voit,[1] from the gain or loss of weight of the

[1] The accuracy of weighing of these distinguished investigators was evidently not sufficient for the purpose.

subject, and to compare the two results thus derived by two independent methods. Or, having computed in the manner already indicated the amount of oxygen absorbed from the atmosphere, we are to calculate the gain or loss of weight of the person or animal which is the subject of the experiment. And if this weight is ascertained by an accurate balance at the beginning and end of the experiment, we have a means of checking by the difference the accuracy of the experiment. But if the subject is weighed four times each day, that is at the beginning of each six-hour period, and the observed weight tallies each time closely with the computed weight, we have a still better verification of the work. The probability is that considerable discrepancies would appear between the observed and computed weights, which would point out the more important sources of error, and thus serve as a means of increasing the accuracy of the work, besides sometimes detecting accidental errors of importance which would not otherwise be noticed.

It was such considerations as these that led to the purchase several years ago of an accurate scale, capable of weighing up to 100 kilos, which was put inside the calorimeter and the subject weighed himself several times during each experiment. However, this check was not applied, and the weighings, though made and recorded were never made use of. Having now retired completely from the investigation, I wish to present here the method which I devised about five years ago and to work it out for the experiment that was published in the recent numbers of the PHYSICAL REVIEW. The weighings were not as accurately nor as frequently made as they might have been; indeed, for this purpose they would better be made by the observers outside, and not by the subject within. I will indicate later how this can readily be done.

Table I, gives the balance of N,C,H,O and ash for the four days taken together. Under each element are two columns headed "In" and "Out," containing respectively the amounts of that element in the income of the body from all sources and the outgo of all kinds. These columns should balance, after the manner of the debit and credit columns of a ledger account, when one has inserted the balancing item. In the case of nitrogen this is the amount of nitrogen acquired from the protein of the tissues which was broken down

TABLE I.

Balance of Nitrogen, Carbon, Hydrogen and Oxygen.

Materials of Income and Outgo.		Total.	Nitrogen.		Carbon.		Hydrogen.		Water.		Oxygen.		Ash.	
			In.	Out.	In.	Out.	In.	Out.	In.	Out.	In.	Out.	In.	Out.
		Grams.												
Food, Water.	Income.	3684.0					412.0		3684.0		3272.0			
" Solids.	Income.	2208.0	76.4		1046.2		150.4				845.8		89.2	
Water of Drink.	Income.	5382.0					602.0		5382.0		4780.0			
Oxygen from Air.	Income.	2611.6									**2611.6**			
Feces, Water.	Outgo.	308.8						34.5		308.8		274.3		
" Solids.	Outgo.	115.2		5.0		53.4		7.3				30.5		19.0
Urine, Water.	Outgo.	6421.0						718.2		6421.0		5702.8		
" Solids.	Outgo.	281.4		73.7		50.3		13.4				68.0		76.0
Water of Respiration.	Outgo.	3765.0						421.1		3765.0		3343.9		
Carb. Dioxide of Res.	Outgo.	3279.8				894.5						2385.3		
From Comb. of Body Protein.		14.4	**2.3**		7.6		1.0				3.5			
" Storing of Body Fat.		72.7				**55.6**		8.7				8.4		
" Dimin. of Body Water.		338.1					**37.8**		**338.1**		300.3			
" Oxid. of Org. Hydro. of food, less that in feces, Urine and Stored.		150.4+1.0 −(7.3+13.4 +8.7)=122.0							1090.7				**5.8**	
			78.7	78.7	1053.8	1053.8	1203.2	1203.2	10494.8	10494.8	11813.2	11813.2	95.0	95.0

over and above what has been formed; for carbon, the amount contained in the fat stored away during the experiment; for hydrogen, the amount derived from the decrease of the water contained in the body; for oxygen, the amount derived from the atmosphere; and for ash, the loss of mineral matter of the body. These balancing items are printed in bold faced type.

The daily ration weighed 1,473 g., of which 921 g. was water and 552 g. dry solids. Thus in four days the water amounted to 3,684 g. and the dry solids to 2,208 g. These quantities are entered in the table in the column of totals. Taking the atomic weights of hydrogen and oxygen as 1 and 15.88, water contains 11.185% of hydrogen and 88.815% of oxygen. The water of the food, therefore, contains 412 g. of hydrogen and 3,272 g. of oxygen, and these items are entered in the proper columns under hydrogen and oxygen respectively. Analysis of samples showed that the dry residue contained 76.4 g. of nitrogen, 1,046.2 g. of carbon, 150.4 g. of hydrogen, and 89.2 g. of ash. The remainder, 845.8 g., is oxygen. All these items are entered in the table in their appropriate places. The 5,382 g. of water drunk (more than half of this was in coffee), in addition to that contained in the food, consisted of 602 g. of hydrogen and 4,780 g. of oxygen. The amount of water is also entered in the column of water. The fourth item of income, oxygen received from the air, is entered last of all, being a balancing item.

We get an approximate check upon the oxygen of the food in the following manner. The dry residue contained:

Protein, ...	478.4 g.,	of which	24.0%	is oxygen	=	114.8 g.
Fat, ...	276.0 "	"	11.5%	"	=	31.6
Carbohydrates, ...	1364.4 "	"	50.0%	"	=	682.2
Ash, ...	89.2 "					0.
	Total = 2208.0 g.			Total oxygen	=	826.6 g.

As these percentage compositions are not exact we cannot regard this as more than a rough check upon the direct analyses, although the true value may perhaps lie between the two.

The next four lines in the table give the amounts of the various elements in the solid and liquid excreta, the total weights of which

are inserted in the column of totals, and the quantity of each element in the "out" column of that element. The water and carbon dioxid of respiration in like manner go into the column of output, the latter being exactly $\frac{3}{11}$ carbon and $\frac{8}{11}$ oxygen; that is, the atomic weight of carbon is 11.91 if oxygen is 15.88, or 12 if oxygen be taken at 16.

We now come to the balancing items:

First, the output of nitrogen is 2.3 g. more than the income. We therefore insert 2.3 as the balance, and this multiplied by 6.25 gives 14.4 g. as the weight of protein lost. This is entered in the column of totals and 7.6 g. of carbon, 1 g. of hydrogen and 3.5 g. of oxygen also contained in the protein are charged into the debit side of those accounts.

Second, the receipts of carbon exceed the output by 55.6 g. Hence we insert this as the balancing item and put 72.7 g. in the column of totals as the weight of the fat in which this carbon is stored up; 8.7 g. of hydrogen and 8.4 g. of oxygen are also contained in this fat, and these items are accordingly entered in their proper places.

Third, the output of hydrogen exceeds the receipts by 37.8 g. This is explained by the fact that the amount of water in the body must have decreased during the experiment by 338.1 g., for no other source of hydrogen remains to be considered. This is indeed the only way of ascertaining the change in the content of water in the body. The accuracy of this determination of course depends upon the accuracy of the several items occurring in the hydrogen columns. There is reason to believe they are less accurate than those of the nitrogen and carbon accounts, but the result of this computation is to give a check which tests the hydrogen account more than any other in the table.

Fourth, the item of 338.1 g. of water lost must be entered in the debit column of the water account, and 300.3 g. in the oxygen account. It represents the amount derived from the body's capital stock at the beginning of the experiment. To balance the water account we must take note of the fact that the organic hydrogen of the food when it is oxidized forms water, which must be charged into the debit side of the water account; or rather, the excess of hydrogen

contained in the food (150.4) and that contained in the protein used up (1.0), over that contained in the feces (7.3), urine (13.4) and that stored up in the fat accumulated (8.7). Thus: $150.4 + 1 - (7.3 + 13.4 + 8.7) = 122.0$ g. of hydrogen, which has been oxidized and formed 1090.7 g. of water. Entering this item on the debit side of the water account, the latter balances exactly.

Fifth, the oxygen account is now complete except the balancing item of 2611.6 g., which represents the quantity of oxygen derived from the atmosphere. This assumes that whatever quantity of oxygen is stored up as such or as oxyhaemoglobin in the blood and in the tissues does not vary by an appreciable amount; or, at least, that it was sensibly the same at 7 o'clock on the morning that the experiment began that it was at 7 o'clock on the last morning. This shows an absorption of 27.2 g. per hour or .45 g. per minute.

The balance of oxygen may be put in another form by omitting from both sides of the account the oxygen from the water which is received into the body. It then appears as follows:

BALANCE OF OXYGEN FOR FOUR DAYS.

	Receipts.	Output.
From the food	845.8	
From 14.4 g. protein of the body	3.5	
Excreted in feces (solids)		30.5
" urine "		68.0
" carbon dioxid		2385.3
" water from organic hydrogen		968.7
Stored away in fat		8.4
Balance from atmosphere	2611.6	
	3460.9	3460.9

Sixth, the column of ash does not balance as closely as it ought. There could not be a loss of 5.8 g. of mineral matter, and hence this difference must be mainly due to error in some one or more of the items given. Indeed, the record does not show positively whether all three items were actually determined. A change in any of them would alter the oxygen item by the same amount, but this would affect the final result only slightly, and for the present purpose it makes no difference.

We are now in position to calculate the loss of weight of the person under investigation, during the four days he was confined in the

respiration chamber. We must first note the fact that the feces passed during these four days weighed 498.4 g., being 74.4 g. more than those which belonged to that period, the latter being distinguished by the use of charcoal.

BALANCE OF MATTER.

INCOME.	Grams.	OUTGO.	Grams.
Food	5892.0	Feces	498.4
Drink	5382.0	Urine	6702.4
Oxygen from air	2611.6	Water of respiration	3765.0
Balance = Loss of weight	360.0	Carbon dioxid of respiration	3279.8
	14245.6		14245.6

This loss of 360 grams in weight is made up as follows:

Loss of water from body, see Table I.,	= 338.1 g.
" " feces " "	74.4
" " protein " " " " "	= 14.4
" " ash " " " " "	5.8
	432.7
Gain of fat, " " "	72.7
Net loss of weight, as above,	360.0

Now the weight of the man at the beginning was 68,420 g. and at the end was 68,000[1] g., making a loss of 420 g., or 60 g. more than the calculated loss. This is of course not a very close agreement. The discrepancy may be partly accounted for by the fact the weighings of the subject by himself were not as exact as if made by some one else, and were not made at the exact time of beginning and end. The computed loss of weight is of course in error by an appreciable amount, but for want of more reliable weighings we cannot say definitely what the error is.

Tables II. and III. give the results of a similar computation for each day separately. The food was the same each day, although there was a slight variation of the water of the food and on the first day the water of the drink was about 200 g. more than on the subsequent days. The feces, although passed in only two portions, were assumed to be separated in equal quantity each day. There was only a slight variation from day to day in the evolution of

[1] The record of this weight is 67,200, but from a previous weight it is evident that this is an error and should be 68,000.

TABLE II.

Balance of Nitrogen, Carbon and Hydrogen.—By Days.

Materials of Metabolism.		Nitrogen.							
		1st Day.		2d Day.		3d Day.		4th Day.	
Food, Water.	Income.								
" Solids.	Income.	19.1		19.1		19.1		19.1	
Water of Drink.	Income.								
Oxygen from Air.	Income.								
Feces, Water.	Outgo.								
" Solids.	Outgo.		1.2		1.3		1.2		1.3
Urine, Water.	Outgo.								
" Solids.	Outgo.		18.8		18.7		18.3		17.90
Water of Respiration.	Outgo.								
Carb. Dioxide of Res.	Outgo.								
From Comb. of Body Protein.		0.9		0.9		0.4		0.1	
" Storing of Body Fat.									
" Dimin. of Body Water.									
" Oxid. of Org. Hydro. of food, less that in feces, Urine and Stored.									
Loss of Ash.									
		20.0	20.0	20.	20.	19.5	19.5	19.2	19.2
Protein Consumed :		5.65		5.65		2.5		0.6	
								Total = 14.4	

Materials of Metabolism.		Carbon.							
		1st Day.		2d Day.		3d Day.		4th Day.	
Food, Water.	Income.								
" Solids.	Income.	261.5		261.5		261.6		261.6	
Water of Drink.	Income.								
Oxygen from Air.	Income.								
Feces, Water.	Outgo.								
" Solids.	Outgo.		13.3		13.3		13.4		13.4
Urine, Water.	Outgo.								
" Solids.	Outgo.		12.8		12.8		12.5		12.2
Water of Respiration.	Outgo.								
Carb. Dioxide of Res.	Outgo.		226.7		221.8		220.7		225.3
From Comb. of Body Protein.		3.0		3.0		1.3		0.3	
" Storing of Body Fat.			**11.7**		**16.6**		**17.3**		**11.0**
" Dimin. of Body Water.									
" Oxid. of Org. Hydro. of food, less that in feces, Urine and Stored.									
Loss of Ash.									
		264.5	264.5	264.5	264.5	262.9	262.9	261.9	261.9
Fat Stored :		1.52		21.7		21.4		14.4	
								Total = 72.7	

TABLE II.—*Continued.*

Materials of Metabolism.		Hydrogen.							
		1st Day.		2d Day.		3d Day.		4th Day.	
Food, Water.	Income.	103.0		102.8		103.0		103.2	
" Solids.	Income.	37.6		37.6		37.6		37.6	
Water of Drink.	Income.	167.0		144.3		145.4		145.3	
Oxygen from Air.	Income.								
Feces, Water.	Outgo.		8.6		8.6		8.7		8.6
" Solids.	Outgo.		1.9		1.8		1.8		1.8
Urine, Water.	Outgo.		199.5		213.1		161.3		144.3
" Solids.	Outgo.		3.4		3.4		3.3		3.3
Water of Respiration.	Outgo.		107.1		99.7		101.3		113.0
Carb. Dioxide of Res.	Outgo.								
From Comb. of Body Protein.		0.4		0.4		0.2		0.0	
" Storing of Body Fat.			1.8		2.6		2.6		1.7
" Dimin. of Body Water.		**14.3**		**44.1**			**7.2**		**13.4**
" Oxid. of Org. Hydro. of food, less that in feces, Urine and Stored.									
Loss of Ash.									
		322.3	322.3	329.2	329.2	286.2	286.2	286.1	286.1

TABLE III.

Balance of Water, Oxygen and Ash.—By Days.

Materials of Metabolism.		Water.							
		1st Day.		2d Day.		3d Day.		4th Day.	
Food, Water.	Income.	921.0		919.0		921.0		923.0	
" Solids.	Income.								
Water of Drink.	Income.	1492.8		1289.9		1299.5		1299.8	
Oxygen from Air.	Income.								
Feces, Water.	Outgo.		77.2		77.2		77.2		77.2
" Solids.	Outgo.								
Urine, Water.	Outgo.		1783.6		1906.0		1440.9		1290.5
" Solids.	Outgo.								
Water of Respiration.	Outgo.		957.9		891.7		905.9		1009.5
Carb. Dioxide of Res.	Outgo.								
From Comb. of Body Pro.									
" Storing of Body Fat.									
" Dim. of Body Water.		**127.8**		**396.0**			**65.6**		**120.1**
" Ox. of Or. Hydro. of food, less that in feces, Ur. and Stored.		277.1		270.0		269.1		274.5	
Loss of Ash.									
		2818.7	2818.7	2874.9	2874.9	2489.6	2489.6	2497.3	2497.

TABLE III.—*Continued.*

Materials of Metabolism.		Oxygen.							
		1st Day.		2d Day.		3d Day.		4th Day.	
Food, Water.	Income.	818.0		816.2		818.0		819.8	
" Solids.	Income.	211.5		211.5		211.4		211.4	
Water of Drink.	Income.	1325.8		1145.6		1154.1		1154.5	
Oxygen from Air.	Income.	**664.3**		**645.9**		**642.6**		**658.8**	
Feces, Water.	Outgo.		68.6		68.6		68.6		68.5
" Solids.	Outgo.		7.6		7.6		7.6		7.7
Urine, Water.	Outgo.		1584.1		1692.9		1279.6		1146.2
" Solids.	Outgo.		17.3		17.3		16.8		16.6
Water of Respiration.	Outgo.		850.8		792.0		804.6		896.5
Carb. Dioxide of Res.	Outgo.		604.4		591.6		588.6		600.7
From Com. of Body Pro.		1.4		1.4		0.6		0.1	
" Storing of Body Fat.			1.7		2.5		2.5		1.7
" Dim. of Body Water.		113.5		351.9			58.4		106.7
" Ox. of Or. Hydro. of food, less that in feces, Ur. and Stored.									
Loss of Ash.									
		3134.5	3134.5	3172.5	3172.5	2826.7	2826.7	2844.6	2844.6

Materials of Metabolism.		Ash.							
		1st Day.		2d Day.		3d Day.		4th Day.	
Food, Water.	Income.								
" Solids.	Income.	22.3		22.3		22.3		22.3	
Water of Drink.	Income.								
Oxygen from Air.	Income.								
Feces, Water.	Outgo.								
" Solids.	Outgo.		4.8		4.8		4.7		4.7
Urine, Water.	Outgo.								
" Solids.	Outgo.		19.4		19.4		18.8		18.4
Water of Respiration.	Outgo.								
Carb. Dioxide of Res.	Outgo.								
From Comb. of Body Pro.									
" Storing of Body Fat.									
" Dim. of Body Water.									
" Ox. of Or. Hydro. of food, less that in feces, Ur. and Stored.									
Loss of Ash.		**1.9**		**1.9**		**1.2**		**0.8**	
		24.1	24.1	24.1	24.1	23.6	23.6	23.2	23.2

carbon dioxid, but a greater difference in the water of respiration and a still greater variation in the urine. The quantity of the latter was so great the first two days that the total amount of water in the body decreased 523.8 g., but during the last two days it increased 185.7 g. The quantity of oxygen derived from the air varies but little, and in nearly the same degree as the evolution of carbon dioxid. The quantity of both oxygen and carbon dioxid was greater on the first and fourth days than on the two intervening days. The ratio of the carbon dioxid exhaled to the oxygen absorbed, called the respiratory quotient, is so important that I have put down the value for each day.

	1st Day.	2d Day.	3d Day.	4th Day.		Total.
Carbon dioxid exhaled,	831.1	813.4	809.3	826.0		3279.9
Oxygen absorbed,	664.3	645.9	642.6	658.8		2611.6
Respiratory quotient,	1.251	1.259	1.259	1.254	Ave.,	1.256

The respiratory quotient for the first day is 0.4% less than the average, corresponding to 2.6 g. of oxygen: that of the second and third days is 0.2% more, corresponding to 1.6 g. of oxygen, while that of the fourth day differs still less, corresponding to only 1.1 g. of oxygen. We know that the respiratory quotient should not be exactly constant from day to day in this experiment, because the receipts and expenditures of oxygen aside from respiration varied slightly. But these variations were only slight, and the nearly constant value of the respiratory quotient is an evidence of the correctness of the values of the oxygen absorbed from the atmosphere, as calculated in the table.

Table IV. gives the balance of matter for each day of the experiment and the calculated gain or loss each day. For the first day there is a discrepancy of 145.4 g. This indicates either a serious error in weighing or else a large error in the water account of that day. For the second day the difference is — 42.6 g., the third day practically nothing, the fourth day — 42.1 g., the total for the four days being 60 g. If the weighings had been more accurate and at exactly 7 o'clock each morning, the result would be more conclusive. But the present purpose is to show how this check may be made in future, rather than to seek to confer any benefits upon past experiments.

TABLE IV.

Balance of Matter.

		1st Day.	2d Day.	3d Day.	4th Day.
Income.	Food, Water.	921.0	919.0	921.0	923.0
	" Solids.	552.0	552.0	552.0	552.0
	Drink, Water.	1492.8	1289.9	1299.5	1299.8
	Oxygen of Air.	664.3	645.9	642.6	658.8
Outgo.	Feces.	——	216.6	——	281.8
	Urine.	1855.3	1977.6	1510.6	1358.9
	Water of Respiration.	957.9	891.7	905.9	1009.5
	CO_2 of Respiration.	831.1	813.4	809.3	826.0
	Balance.	**14.2**	**492.5**	**189.3**	**42.6**
		3644.3–3644.3	3899.3–3899.3	3415.1–3415.1	3476.2–3476.2
	By Scales.	Lost 160.0	Lost 450.0	Gained 190.0	Lost 0.0
	By calculation above.	" 14.6	" 492.6	" 189.3	" 42.1
	Difference.	+145.4	−42.6	−0.7	−42.1

Difference for four days 60 grams, lost more by scales than by calculation.

But one can also calculate the weight of the subject at other times of the day and additional checks may thus be instituted which might be of service in locating errors and deriving the largest possible amount of information from an experiment. Suppose that it is desired to calculate the weight of the subject at the beginning of each 6-hour period, that is at 7 A. M., 1 P. M., 7 P. M. and 1 A. M. of each day. The CO_2 and H_2O of respiration are separately determined for each such period any way, and it is only a matter of slight computation to get all the data for a check upon the work four times each day, having the weight of the subject by an accurate scale at the same times.

In computing the fat gained or lost from the gain or loss of carbon, it is assumed that the glycogen and other carbohydrates of the body at 7 A. M. each day are constant in quantity, the diet being uniform from day to day. In order that the same assumption might hold true, or approximately so, for the six-hour periods it would be desirable to serve the meals just after the beginning of the periods. Then the glycogen content of the liver and tissues would be more nearly uniform than if supper were served at 6:30, as was done in these experiments. Even then, of course, it would vary,

but the error introduced by the probable variation would not be considerable.

TABLE V.

Table of Gain and Loss.

	1st Day.		2d Day.		3d Day.		4th Day	
	Gain.	Loss.	Gain.	Loss.	Gain.	Loss.	Gain.	Loss.
Water.		127.8		396.0	65.6		120.1	
Feces.	106.0			110.6	106.0			175.8
Protein.		5.7		5.7		2.5		0.5
Fat.	15.2		21.7		21.4		14.4	
Ash.		1.9		1.9		1.2		0.8
Balance.	**14.2**		**492.5**			**189.3**	**42.6**	
	135.4	135.4	514.2	514.2	193.0	193.0	177.1	177.1

In the accompanying figures a graphic representation of the change of weight of the person during the 96 hours of the experiment is given. The points representing the weights at the end of every 6 hour period are inclosed in small circles, and the weight as computed is given in figures. Between these points the values are not exact, for it has been assumed that the evolution of H_2O and CO_2 and the absorption of oxygen were uniform during each period. Of course these quantities are greater during the exercise incident to dressing and eating than during subsequent rest, but for the purpose of illustrating the fluctuation of weight during a day, or during four successive days, this variation may be neglected. It would be of very great interest if we had the precise weight of the subject at each of these 17 six hour stations. Another source of uncertainty in the calculated weights at the six hour stations is that the respiratory quotient, determined above for each day separately, was assumed to be constant for the different periods of each day. But in experiments carried out with that in view it might be separately computed for each period. That might give information of value in itself as showing to what extent oxygen may be stored up in the body.

The chart is mainly self-explanatory. Starting at 7 o'clock on the morning of the first day with a weight of 68,420 g. the subject loses 45 g. in one hour by respiration. That is, the loss of CO_2

and H_2O above the gain of oxygen by respiration is 45 g. per hour during the first 6 hours. Breakfast adds 675 g. to his weight.

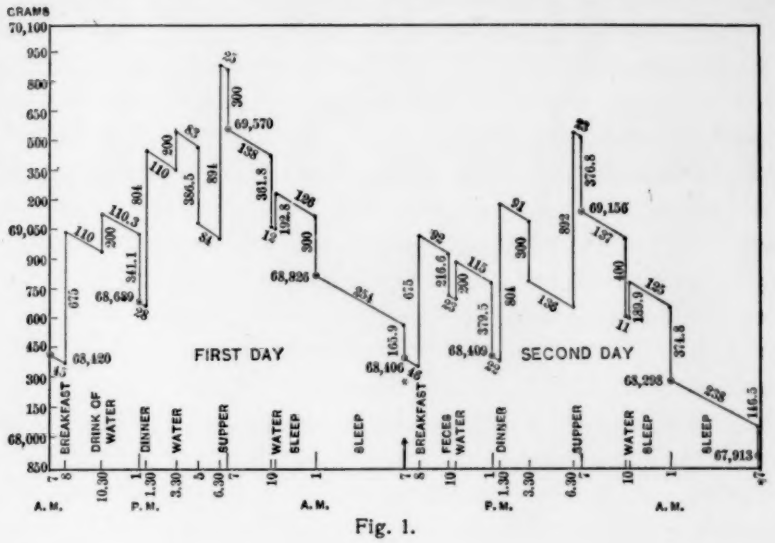


Fig. 1.

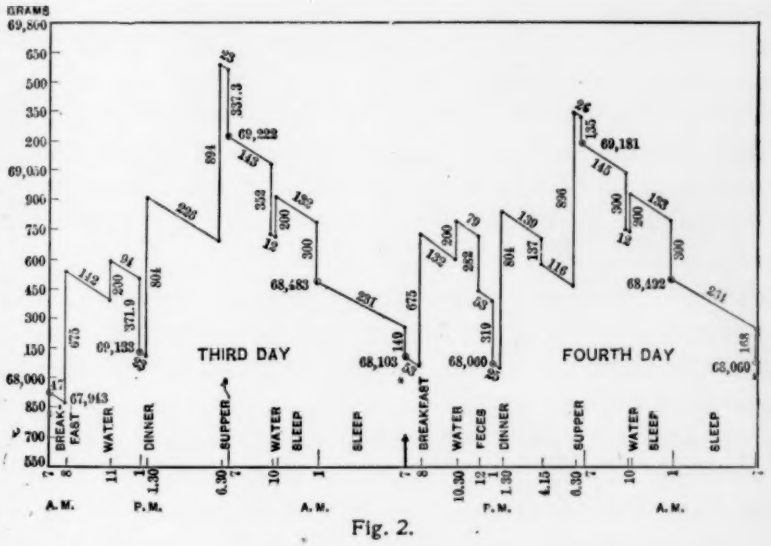


Fig. 2.

Of course the gain of weight due to breakfast was not instantaneous, but it was not deemed necessary or desirable to indicate the time occupied in eating meals by a sloping line. During the next 2½

hours until 10:30 a loss of 110 g. occurred by respiration, and then a drink of water increased the weight by 200 g. A further loss of 110.3 g. by respiration was followed by a decrease due to passage of urine, and at one o'clock the weight should have been 68,689 g. We have no observed weight to compare with it, but the weights we have at seven o'clock each morning are plotted in the chart as stars.

The charts show very clearly the considerable fluctuations of weight throughout a period of 24 hours. The minimum weight is at eight o'clock, just before breakfast, and the maximum just after supper. The slope of the lines showing the loss of weight by respiration is nearly uniform, as the loss of weight is nearly as rapid at night during sleep as during the day. The evolution of CO_2 is only slightly more than half as much at night as during the day (and in a work experiment only about one-fourth) but the absorption of oxygen is also less in (nearly) the same ratio. On the other hand, the rate at which water is given off from the skin and lungs is nearly as great at night as in the day in a rest experiment (although far less at night than during the day when at work). Hence while there was a loss of 265 g. by respiration in 6 hours, from 7 a. m. to 1 p. m. on the first day, there was a loss of 254 g. from 1 a. m. to 7 a. m. during sleep.

While the check that is thus afforded by knowing the weight of the subject at intervals during the experiment would not detect small errors in the determination of nitrogen and carbon, it would detect appreciable errors in the determination of the water, and this is the quantity most in need of such a check. Considering the great labor and cost of complete respiration experiments it appears of the greatest importance to use every possible means to reduce errors to a minimum and to derive the maximum amount of information from each experiment.

In order to weigh the subject with the greatest possible accuracy, he should be seated quietly on the platform of the balance, and the weighing done from without. This may be accomplished by using a rider, controlling it by a rod from outside, in a manner similar to that in a chemical balance. The subject would set the heavy weight at such a point that the beam nearly balanced. The exact balance would then be secured by the rider. The beam should be visible

from without through the window, so that readings of the weight and rider could be made by the observer outside. The subject ought, however, to note and report the reading, when the weighing is finished, as a check upon the record of the observer. The position of exact balance could be obtained by use of a reading telescope, set in the walls of the chamber and a graduated scale attached to the beam would swing to and fro in the field. By observing the range, very close settings could be made, and with a good Fairbanks balance readings could be made at least to a single gram.

In order that these close readings be as reliable as possible, several successive weighings should be made. The subject is constantly growing lighter by the losses due to respiration. Hence the rider could be set a little to one side of a balance, and the time noted when exact balance was secured. A second setting would give a balance, say about a minute later. Let five such weights be made, extending before and after the time at which the weight is desired, and from the mean the true weight could be obtained to within a gram at least, not counting any constant error due to inaccuracy of the balance, which would be of no moment in taking the difference.

The subject should have a light suit of clothes, without pockets, and be weighed with his clothes on. Any moisture contained in his clothing would then be reckoned as part of himself. In a rest experiment this would be small, and the disturbance due to the exercise of dressing and undressing would be avoided.

Mechanical Work.

The bicycle-dynamo combination, which we employed to develop and measure mechanical energy at the expense of the potential energy of food and tissue was not the ideal form of ergometer. It was easily arranged and served our purpose for the time being. But it required calibration and the energy expended in friction was somewhat variable; moreover, it is probably not a machine in which the maximum efficiency of conversion could be effected. A better arrangement would be a simple treadmill. That is, a movable inclined plane consisting of slats attached to two endless chains, the latter running over sprocket pulleys. A fly wheel to steady the motion might consist of a heavy copper disc revolving between the

poles of permanent magnets, thus serving also as a brake. By varying the position of the magnets the braking effect and, therefore, also the rapidity of motion could be varied, and by varying the inclination of the plane greater or less force would be exerted. The three quantities required in order to calculate the work done in any given time would be (1) the angle of elevation of the plane, (2) the whole number of revolutions of the flywheel or sprocket pulley and the distance traveled by the plane for each such revolution, and (3) the weight of the person operating the mill. The first is easily determined by reading a graduated sector, the base being level; the second would be recorded by a simple counter; the third should be frequently determined for purposes of a check upon the observations and analyses, and hence would not involve any additional labor. This form of ergometer would enable a large or small amount of work to be done, would give a high efficiency of conversion, would require no calibration, would involve no error due to varying friction, would require no record of varying speed to be made, and could be constructed at a reasonable cost. With it a large variety of experiments could be made.

The conversion of the energy of food and tissue into mechanical energy is a profound mystery. We cannot understand how a contracting muscle transfers the energy liberated by the oxidation of matter into an outside portion of matter. Perhaps the heat produced by the oxidation is transformed by some process equivalent to that of a heat engine. If the second law of thermodynamics does govern the process, it may be that the requisite difference of temperature occurs only in spaces of the order of magnitude of molecules, and hence cannot be measured experimentally. Or it may be that the oxidation is accompanied by an evolution of less heat when work is done, as is the case when zinc is consumed in a battery furnishing a current of electricity. In other words, that it is in some way electrical, and hence that no such difference of temperature exists as would be required in a heat engine or its equivalent. Some such theory has been put forward, although it is far from explaining the process. It is possible that further experiments such as can be carried on with the respiration calorimeter will give such additional information as at least to assist in formulating a more satisfactory theory of the process.

But there is another case of interest which demands experimental examination. That is the converse case where work is done upon a man, for example, by an outside agent. If a laborer spends a day in hoisting brick to the top of a building by means of a windlass, the heat which his body gives out plus the work done upon the brick will equal the net potential energy of food and body tissue used up in the same time. Now suppose that next day he lowers the same brick to the ground again. The work done is negative in the mechanical sense, although the force exerted in handling the windlass is nearly as great as in hauling them up. Will the heat evolved *minus* the loss of potential energy of the brick equal the potential energy of food and body tissue used up? And will the heat yielded by the body be more or less than on the preceding day?

Or, again, suppose the windlass is driven by an engine while the laborer grasping the handle holds back with all his might. The engine does a large amount of work upon the man. What is the form that this energy assumes? Is oxidation checked by the exertion of force upon muscle?

Or, again, suppose a man is walking down hill. If he weighs 150 pounds and walks 10 miles down a grade of 14½° he will descend vertically 2½ miles and the potential energy given up by his body as a whole will be 1,980,000 foot pounds, which is equivalent to a horse-power working for an hour. What form does this energy take? Such an experiment could be performed in a respiration calorimeter by using the proposed treadmill for an inclined plane, and driving it by an electric motor, having the man walk down hill as the plane was driven up. Perhaps such experiments have already been made, but so far as I am aware no satisfactory answers to these questions have been reached.

An intermediate case is where force is exerted without doing work, as when a weight is held in the hands without being raised or lowered.

The suggestion that there is an energy-bearing product of mental effort which is capable of being apprehended by physical instruments is contrary to our physical notions. The increased generation of heat in the brain and head, due to increased circulation of

the blood or increased oxidation of tissue or other oxidizable material during mental activity is however something that can be studied. But it would require special apparatus other than, or in addition to, the respiration calorimeter.

WESLEYAN UNIVERSITY, MIDDLETOWN, CONN., September 5, 1899.

A DISSOCIATION THEORY OF THE ELECTRIC ARC.

By C. D. Child.

A SERIES of very instructive photographs of the alternating current arc were shown by Mr. N. H. Brown in the Physical Review for November, 1898. An experiment recently performed by the present writer bears somewhat upon some of the phenomena there noticed and it may not be amiss to review briefly the results of Mr. Brown's article before describing this experiment. In order to refer more easily to the photographs Plate II. of that article is here reproduced. These are photographs of the alternating current arc taken upon a rapidly moving film. A screen with a narrow slit was so placed that light from but a small section taken lengthwise of the arc fell on the film. The films moved from left to right, and accordingly when read from right to left, they give the history of the arc through successive alternations. There appear to be two light fronts, one starting somewhat sooner than the other. As indicated by Mr. Brown, it is the negative one which starts the sooner, but moves with the slower velocity. The different photographs were taken with different velocities of the film and with different distances between the carbons but the general characteristics are the same in all here reproduced.

There would seem to be no doubt as to the cause of the fact that the arc starts at the negative carbon. As was shown the carbon which is negative at the beginning of a passage of the current was in the previous discharge positive, and it is well known that after the current has once been established the positive carbon is the hotter. This carbon would continue to be the hotter after the previous discharge had ceased, the vapors in its immediate vicinity would continue to be hotter than elsewhere and the current would accordingly begin at this point.

The explanation of the fact that the positive light front appears to move the more rapidly is not so simple. But though not simple,

it may be the means of giving us a much better knowledge of the arc than that which we now possess. The explanation was suggested that the current was at least in part carried by ions and that the positive ions move with the greater velocity. Mr. Brown does not appear to consider this a probable explanation, and in fact it does not seem to be a possible one in the form suggested, *i. e.*, that the greater velocity of the positive ions may be due to the greater fall of potential at the positive carbon. For the distance through which the positive ions have the greater velocity is shown by the photographs to be greater than the space in which the fall of potential is so great and millions of times greater than the mean free path of the molecules at atmospheric pressure. We cannot see how the mean free path of the ions carrying the current can be greater than that of ordinary molecules. The ions will not maintain their velocity more than the distance of their mean free path beyond the space where the fall of potential is so great, unless the air move as one mass, but the photographs show that there is a motion from the negative to the positive as well as from the positive to the negative, and the air, as a whole, cannot move in both directions at once.

There is, of course, the possibility that the effects shown in the photographs are not caused by ions of molecular size, but particles of carbon which are driven off. But if this were the case, we should expect a disturbance of some kind at the point where the streams of negative and positive particles meet, and that this would be shown in the photographs. Nothing of this sort, however, is noticeable.

There are indications in the photographs of a phenomenon which would afford a still stronger reason against the idea that the greater velocity is caused by the greater fall of potential at the positive carbon. In one or two cases, especially in the discharge shown at the left hand of *2A* the negative appears to move slower at the same time and in the same region where the positive front is moving more rapidly. If this should be shown to be the case, it would afford very strong evidence that the current is carried by ions, and that the positive ones move the more rapidly, not because of the greater fall of potential at the positive carbon, but because they do so even with the same potential gradient.

It is not known to the present writer whether it is possible to

PLATE II.

2A

2B

2C

2D

2E

2F

N. H. BROWN. PHOTOGRAPHIC STUDY OF THE ELECTRIC ARC.

[To accompany an article by C. D. CHILD.]

make photographs of such a character as to decide definitely this point, and in the absence of such knowledge it seemed well to consider the conclusions to which such a hypothesis would lead. Moreover the experiment to which reference has been made tends to substantiate this view. The following article will accordingly be devoted, first, to a discussion of the hypothesis that the current is carried in the arc by ions and that the positive ions with the same potential gradient move the more rapidly; and second, to a description of the experimental work performed.

As is well known, this resistance of the arc is not an ohmic resistance. A minimum difference of potential is necessary in order to maintain an arc, and when the current is increased the apparent resistance becomes smaller. Indeed, the resistance decreases so rapidly in the case of poles of solid carbons with a definite distance between them that the difference of potential becomes smaller as the current becomes greater.[1] The greater part of the fall of potential through the arc occurs at the surface of contact between the conducting gas and the carbons, and by far the greater fall is at the positive pole. Diagram 1 represents the fall of potential between two poles according to the data found by Mrs. Ayrton.[2] The continuous line would represent the case when a current of 10 ampères flows between the solid carbons at a distance from each other of 8 cm. The explanation often offered is that there is a counter E. M. F. in the arc caused by a combination of the Thomson and Peltier effects. However, it has never been possible to find any trace of the counter E. M. F. after the impressed E. M. F. has been removed. This is surprising since the gases of the arc can be shown to be conducting for an appreciable time after the current has ceased to flow, and a counter E. M. F. acting for 1/200 sec. would surely be detected.[3] If the effect is a true Thomson effect, it will last as long as the inequalities of temperature continue. That the inequalities do last for an appreciable time is shown by photographs of the arc. There may, of course, be some action similar to the Thomson effect but in just what the similarity consists has not yet been shown.

[1] Lond. Elec., Vol. 34, p. 366.
[2] Lond. Elec., Vol. 41, p. 720.
[3] Lond. Elec., Vol. 39, p. 615.

On the other hand the suggestion that the current in the arc is carried by ions and that the positive ions have the greater velocity would appear to offer a satisfactory explanation of the arc. A similar hypothesis was given by Prof. Zeleny to account for the phenomena in the case of discharge produced by X-rays.[4] His hypothesis needs but little modification to adapt it to the case in hand. The principal difference would be that in the case described by Prof. Zeleny the negative ions had the greater velocity, while in the case in hand the positive ions have the greater velocity.

We must suppose that the intense heat of the arc dissociates the gases of the arc into positive and negative ions. We cannot at

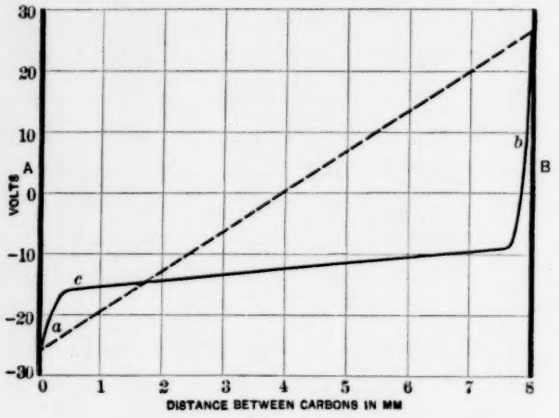


Fig. 1.

present know whether the charges on the oppositely charged ions are or are not equal. But in any case it would not alter the essential idea of this article, and for simplicity we may assume an equal number of positive and negative ions with equal charges on the two kinds of ions. On being dissociated the positive ions move toward the negative pole, and the negative ions towards the positive pole. No doubt recombination and again dissociation take place frequently. But however often this may occur there will be an action equivalent to a motion of the negative ions in one direction and the positive in the opposite direction.

There would be an excess of positive ions in the vicinity of the

[4] Phil. Mag. (5), Vol. 46, p. 120.

negative pole and of the negative ions in the vicinity of the positive pole somewhat as indicated in Fig. 2, where the negative pole is at the left. The amount of electrification is shown by the ordinates. This electrification would produce an effect on the potential. Without any electrification the potential would fall regularly from one pole to the other as shown by the dotted line in Fig. 1. The electrification would change it so as to conform roughly with the continuous line in Fig. 1.

If now the positive ions move with the greater velocity, they would in general take less time in reaching the pole toward which they are moving, and there would consequently be fewer of them between the poles at any instance of time. In a similar way if two

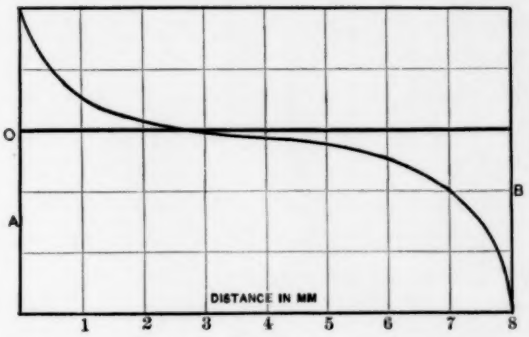


Fig. 2.

armies having equal numbers of soldiers were to march over a given distance, the army moving the more rapidly would afford fewer soldiers per acre. The same is true of the ions. This will be true whether the ions move directly across from pole to pole, or only succeed in going across after many interruptions produced by their dissociating and recombining. The excess of negative ions is indicated in Fig. 2. If then there are more negative than positive ions between the carbon terminals, they would cause the potential of the gas to be lower than it otherwise would be, as is indicated in Fig. 1.

At the surface of contact between the gases and the carbons the conditions are somewhat exceptional. At *a*, Fig. 1, there would be no negative ions moving toward the right. If the potential gradient

were the same here as elsewhere, there would be positive ions at c, which had come from the right, but no negative ions from the left to neutralize the electrostatic effect. The potential at c would, therefore, become positive, due to the positive ions, and the rise of potential between a and c would be greatly augmented. A similar effect would occur at the positive pole. This would account for the large fall of potential at the poles. The fact that the potential of the gas is lowered, because of the greater velocity of the positive ions would make the fall of potential at the positive pole greater than at the negative.

It is impossible to put this explanation into mathematical form, because of the complexity of the conditions. The amount of dissociation at any point depends on the temperature at that point according to some law which is not at present known, and the temperature in turn depends on the potential gradient which in turn depends on the amount of dissociation. It would be wiser to leave this circle of dependencies until our knowledge of the subject is in a more advanced state.

With this explanation of the arc it is easy to see why the apparent resistance becomes less as the current is increased. With increase of current there would be more heat developed in the arc. A greater dissociation would occur, and this in turn would cause the apparent resistance to be less. It would, however, be impossible for the potential difference to become smaller so fast that the amount of heat developed by a large current would be less than that developed by a smaller one. As far as is known this does not occur.

This view also harmonizes with the fact that with an alternating current the average potential between the poles is not zero, but is negative. The positive ions moving with greater velocity pass out of the field quicker, leaving an excess of negative ions and a corresponding negative potential.

The experiment referred to at the beginning of this article was suggested by a recent article in the *Philosophical Magazine*[1] by Professor A. P. Chattock on the velocity of ions in the case of discharge from a point to a plate. From his experiment there appears to be a drag

[1] Phil. Mag. (5), Vol. 48, p. 401.

produced on the air by the friction between the air and the ions, and this drag shows itself as an increased air pressure at the plate. Professor Chattock was able to measure this pressure by a sensitive manometer. It seemed possible to the present writer that if the current of the arc is carried by ions their presence could be detected by a similar method.

The manometer used in this experiment was similar to one shown me at one time by Mr. W. R. Turnbull. I am unable to state whether any description is given of it elsewhere or not. It consists of an air-tight chamber, 4 cm. on each side and 1 cm. thick. One side is covered with rubber dam which moves with a very slight increase of pressure on the air within the chamber. The chamber is shown in cross section in Fig. 3. *A* is the chamber which may be connected at *e* to the other parts of the apparatus. *R* is the rubber dam covering one face. In front of the rubber is the mirror *m*. The mirror is fastened to the needle *n* which is free to rotate. *a* and *b* are strips of tin projecting from the chamber *A* and holding the needle *n* in place. The wire *d* is fastened to the rubber dam and also to the mirror near to the axis about which the mirror rotates, so that a slight motion of *d* gives the mirror a sensible rotation. This rotation was read by a telescope and scale. It has not as yet been possible to do any quantitative work, and accordingly the manometer was only roughly calibrated by comparing it with a water manometer. A pressure of 1 mm. of water corresponded to a deflection of 10 cm. with the sensitive manometer. A deflection of 1 cm. would therefore correspond approximately to a pressure of 1/100,000 of an atmosphere.

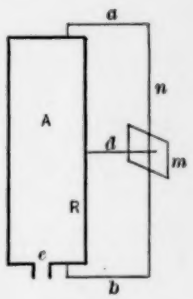


Fig. 3.

A small hole was drilled lengthwise through one of the carbons and the manometer was connected with this aperture.

On starting the arc the manometer showed an increase of pressure. It was, of course, impossible to keep the end of the arc on the same point on the carbon, and as a result there was a continual variation in the pressure indicated. The pressure was greater as the end of the arc was nearer the aperture. This continual varia-

tion of the pressure made any qualitative measurement impossible, but it was plain that the pressure was greater at the positive carbon.

In order to remove any doubt a manometer was constructed so as to measure the *difference* between the pressures at the positive and the negative carbons.

The instrument was the same as that shown in Fig. 3, except that in front of R was placed a second air-tight chamber. A window in this chamber allowed the motion of the mirror to be detected as before. One chamber was connected with the aperture through one carbon, and the other chamber to the aperture of the other carbon.

With this instrument the deflection was to the right or to the left depending on the direction in which the current passed between the carbons. The deflection was always reversed when the current in the arc was reversed. By this it was proved that the pressure at the positive carbon was greater than that at the negative. The deflection of the manometer was at times as much as 5 cm., showing that the difference of pressure was .00005 of an atmosphere.

The effect was very much more marked with a large current than with a small one. Thus when the current was 6 ampères there was scarcely any pressure indicated. When the current was 20 ampères an average reading of the manometer would indicate a difference in pressure of .00002 of an atmosphere. This was possibly due to the fact that the arc in the latter case covered a larger part of the carbon about the aperture.

The carbons were placed horizontal in order to avoid convection currents. But when they were placed other than horizontal no difference in the effect was noticed. Moreover, it seems impossible that the effect could be due to convection currents, because reversing the current without in any way changing the position of the carbons or their connections to the manometer would reverse the effect.

The effect may be due to the expansion of the gases in the arc caused by the heat, perhaps to the formation of CO_2 or to vaporization of the carbon. We should in this case expect the pressure at the positive carbon to be the greater, because the arc there is the

hotter. This is, indeed, what we find. There is one objection to this view. If this effect were due to the heat of the arc, one would expect that when one carbon was directly over the other the upper carbon would show the greater pressure whichever way the current flowed. This was not found to be the case. The positive carbon showed the greater pressure whether it was up or down. Moreover, the experiment was tried of inclosing the carbons in a chamber from which free access of air was excluded as far as possible without stopping all vent for the expansion of the gases caused by the arc. The effect was in this case the same as when free access of oxygen was allowed. These cannot be considered as proofs that the effect is not due to differences of temperature, but they are at least facts which will warrant us in considering some other hypothesis.

Professor Chattock showed that in the case of discharge from a point to a plate $p = \frac{Cz}{V}$ where p is the total drag exerted on the air, C equals the total current, z equals the distance between the point and the plate and V equals the velocity of the ions for unit potential gradient. In the case of the arc the conditions are much more complicated. Instead of the current being all carried by ions of one sign, it is carried by both positive and negative ions. This in itself would not make the problem impossible of solution, if at all points the same part of the current were carried by ions of one sign. This, however, is not the case, and because of irregularities of the arc it is impossible to make any mathematical statement which will apply. But we can get some insight into the matter by considering the physical meaning of the equation $p = \frac{Cz}{V}$. The drag which the ions produce must be equal to the force exerted upon them since their acceleration is negligible. It is as it would be with a block being pulled over the floor with a uniform velocity. The pull on the floor due to the friction is the same as the pull on the block. So the drag on the air is equal to the total attraction between the plate and the ions which are at any instant above the plate. This will evidently be greater the greater the number of ions. Other things being equal the number of ions will be greater the

slower they move. This is shown in the equation $p = \frac{Cz}{V}$. The number will be greater the more current is being carried and will be greater the greater the distance between the point and the plate. The drag will, therefore, be proportional to C and to z.

With this conception of the cause of the drag, *i. e.*, that it is caused by the attraction between the electrodes and the ions, we can apply it qualitatively to the arc. Referring to Fig. 2 we see that there must be a pressure on the positive carbon due to the attraction between the carbon and the negative ions near it. There will also be a pressure on the negative carbon for a similar reason. The attraction on the positive carbon will, however, be the greater because there are more negative ions between the carbons than positive ones. This, indeed, was what was found in the experiment.

It is thus seen that all the phenomena of the arc which are known can be explained in accordance with the hypothesis here suggested. The action of the arc is, however, very complicated, and it may well be that, though this explanation be partially correct, it is not a complete explanation. It is hoped that a more thorough investigation of the phenomena here noticed may bring to light facts more decisive, and result in definite knowledge concerning what has heretofore proved a troublesome problem.

COLGATE UNIVERSITY, January, 1900.

THE EFFECT OF MAGNETIZATION ON THE ELASTICITY OF RODS. II.

By James S. Stevens.

IN the Physical Review for August, 1899, there was described a series of experiments in which by the use of interference methods, it was shown that iron and steel rods developed a greater elastic modulus when magnetized. In those experiments flexional elasticity alone was considered. In the present paper I shall describe some experiments which had for their object the determina-

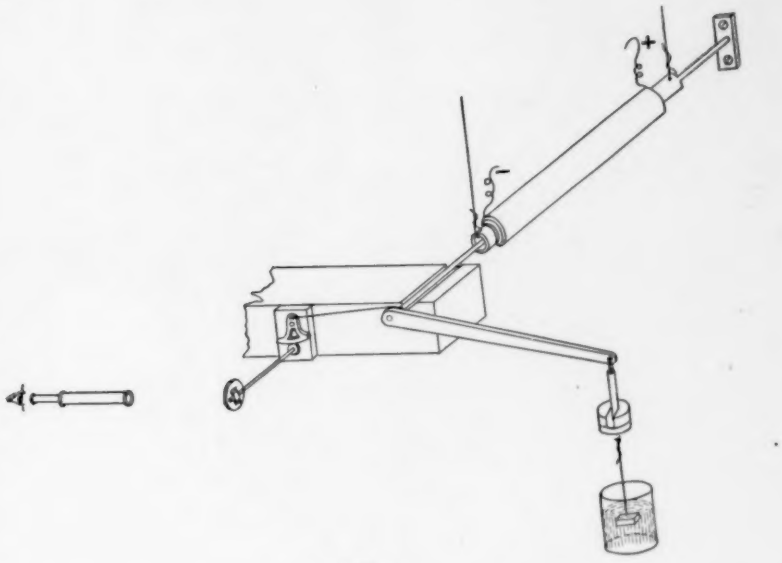


Fig. 1.

tion of the effect of magnetization upon torsional elasticity. That some relation exists between torsion and elasticity has been known since 1847, when Matteucci examined the change in magnetization undergone by an iron rod when it was twisted. Further investigations have been carried on by Wertheim, Wiedemann, Kelvin and others, with the general conclusion that when a rod of soft iron

exposed to longitudinal magnetizing force was twisted, its magnetism was reduced by torsion in either direction (Ewing).

In these experiments three styles of cylindrical rods were employed: one of iron, 183 cm. long and 0.48 cm. in diameter; one of steel of the same dimensions, and lastly the iron rod was reduced in length to 163 cm. To magnetize these rods a coil of two layers of No. 16 wire was wound in 1,612 turns. The length of the coil was 132.1 cm. One end of the rod was rigidly screwed into an ron plate, fastened to a solid wall, and to the other was attached an arm carrying a weight at the opposite end which supplied the force of torsion. The arrangement of the apparatus is shown in the accompanying figure.

After the rod had been twisted by the application of the given weight the current was sent through the coil, and in every case where the current was sufficiently strong a *rising* of the weights was observed, showing that the magnetizing effect was such as to increase the torsional elasticity. In order to damp the vibrations a small weight suspended in a jar of water served as a dash pot. The following table shows approximately the angles through which the rods were twisted by the loads applied.

Moment of Torsion (dynes).	Angle of Torsion (degrees)
6.3×10^6	2
4.8×10^7	15
9.8×10^7	30
13.6×10^7	39

To measure the change which took place in the angle of torsion when the rods were magnetized, I employed two methods: first, a mirror was fastened to the arm and the rotation of the rod was measured by a telescope and scale; secondly, in case the rotation was small, a device shown in the figure was employed which multiplied the scale readings 9.43 times.

In the following tables are shown the current, magnetizing force, scale readings and the angle representing the corresponding rotation of the rods.

TABLE I.

Iron rod, 183 *cm. long. Moment of Torsion* 9.8×10^7 *dynes.*

Magnetizing Force.	Scale Readings.	Angular Rotations (Minutes).	Magnetizing Force.	Scale Readings.	Angular Rotations (Minutes).
7.7	0.01	0.3	50.0	0.94	25.5
13.7	0.02	0.6	54.6	0.97	26.3
19.9	0.12	3.3	71.9	1.1	33.0
23.0	0.29	7.8	76.5	1.2	36.0
29.1	0.45	12.3	79.6	1.2	36.0
32.1	0.66	18.0	91.8	1.2	36.0
35.2	0.72	19.2	104.0	1.3	39.0
38.3	0.74	20.1	117.8	1.3	39.0
42.8	0.83	22.5	127.0	1.3	39.0
44.3	0.87	23.7	137.7	1.4	42.0

TABLE II.

Iron Rod, 183 *cm. long. Moment of Torsion* 4.8×10^7 *dynes.*

Magnetizing Force.	Scale Readings.	Angular Rotations (Minutes).	Magnetizing Force.	Scale Readings.	Angular Rotations (Minutes).
4.6	0.01	0.3	36.7	0.90	24.6
9.2	0.02	0.6	39.8	0.92	25.2
13.8	0.23	6.6	47.4	1.00	27.3
16.8	0.35	9.6	52.1	1.06	28.8
19.9	0.46	12.6	64.3	1.13	30.9
23.0	0.62	16.8	76.5	1.22	33.3
26.0	0.69	18.9	82.3	1.23	33.6
30.6	0.78	21.3	90.3	1.25	34.2
32.1	0.80	21.9			

TABLE III.

Steel Rod, 183 *cm. long. Moment of Torsion* 9.8×10^7 *dynes.*

Magnetizing Force.	Scale Readings.	Angular Rotations (Minutes).	Magnetizing Force.	Scale Readings.	Angular Rotations (Minutes).
7.7	0.1	2.6	76.5	0.2	5.2
15.3	0.1	2.6	91.8	0.3	7.8
22.9	0.1	2.6	137.7	0.3	7.8
30.6	0.1	2.6	145.4	0.3	7.8
35.2	0.1	2.6	153.0	0.4	10.4
38.3	0.2	5.2	168.3	0.4	10.4
58.1	0.2	5.2	214.1	0.5	13.0
61.2	0.2	5.2			

TABLE IV.

Iron Rod, 163 *cm. long. Moment of Torsion* $9.8 \cdot 10^7$ *dynes.*

Magnetizing Force.	Scale Readings.	Angular Rotations (Minutes).	Magnetizing Force.	Scale Readings.	Angular Rotations (Minutes).
12.4	0.05	1.3	79.4	0.4	10.0
19.9	0.15	3.8	107.1	0.4	10.0
27.5	0.2	5.0	130.0	0.5	12.5
30.6	0.2	5.0	137.7	0.5	12.5
38.3	0.3	5.0	153.0	0.5	12.5
61.2	0.3	7.7	183.6	0.6	15.1
68.8	0.4	10.0	198.9	0.6	15.1
76.5	0.4	10.0			

TABLE V.

Iron Rod, 183 *cm. long. Moment of Torsion* 13.6×10^7 *dynes.*

Magnetizing Force.	Scale Readings.	Angular Rotations (Minutes).	Magnetizing Force.	Scale Readings.	Angular Rotations (Minutes).
33.7	0.01	0.6	91.8	0.67	18.3
47.4	0.04	1.2	183.6	1.14	28.2
53.6	0.08	1.8	229.5	1.14	31.2
56.6	0.26	7.2	306.0	1.17	31.8
64.3	0.48	13.2	382.3	1.19	32.4
82.6	0.58	15.9	405.5	1.22	33.3

Measurements were also taken with no weight added to the arm of torsion except the small one used with the dash pot. The moment of torsion was 6.3×10^6 dynes. While a deflection was noted with each magnetizing force used, the scale readings were quite irregular and the zero not constant.

By use of the device for multiplying the scale readings, it is thought that the error in estimation need not exceed eight seconds.

The above curves were plotted with magnetizing force divided by 4 and corresponding angles of rotation expressed in minutes as co-ordinates. *I* is for the steel rod, *II* the short iron rod, *III*, *IV* and *V*, the longer iron rod with moments of force 13.6×10^7, 4.8×10^7 and 9.8×10^7 dynes respectively. Without doubt, the lack of regularity of the lines is due in large measure to errors of observation.

From the general consideration of the experiments, and the inspection of the tables and plots, the following conclusions may, I think, be written down :

1. Magnetization of an iron or steel rod increases its torsional elasticity. This would be expected from the results of the experiments mentioned in the first part of this paper dealing with the effects of torsion upon elasticity.

Fig. 2.

2. The effect is greater in iron than in steel rods of the same dimensions.

3. The increase in elasticity varies with the length of the rod.

If we compare the results of this experiment with those of the experiments on elasticity of flexion, we note a distinct agreement. In the first place the modulus of elasticity is increased in each case when the rod is magnetized ; the increments are fairly proportional to the magnetizing forces ; and they are inversely proportional to the magnitudes of original stresses. So far as elasticity of traction is concerned, the only results published are given in the PHYS-

ICAL REVIEW, Volume II., No. 4. They do not represent exact measurements but are inferences which were suggested in connection with another experiment, and go to show that in this case also magnetization increases the elasticity. It is expected that a little later the effect of magnetization upon a rod subjected to longitudinal pressure will be studied.

It ought to be said that the mechanical arrangements for this experiment are due to H. G. Dorsey, and that in taking the readings I was assisted by S. Sidensparker, instructors in the Department of Physics.

PHYSICAL LABORATORY, THE UNIVERSITY OF MAINE.

THE INFLUENCE OF TEMPERATURE, PRESSURE, USED SOLUTIONS, AND SIZE OF ANODES ON THE DEPOSIT OF SILVER VOLTAMETERS.

By J. F. Merrill.

THE object of the work herein described was to see if by a change of temperature or pressure, etc., the electric charge carried by silver ions in the electrolysis of silver nitrate could be varied. It is well known that the amount of the silver deposit, as obtained in ordinary electrolysis, varies slightly according to the conditions under which the deposit is obtained. But that this variation is due to any change in the carrying capacity of the silver ions has probably not been proved.

The method here employed was of course comparative. Two silver voltameters, usually alike in all respects, were placed in series and the deposit from one under ordinary conditions compared with the deposit from the other obtained under different conditions. Then any differences in the two deposits must be due to the different conditions. After formation, the two deposits were treated alike with respect to washing, heating, etc., to avoid any variation in amount due to different treatment.

Influence of Pressure.

Preliminary experiments having shown that the pressure must be considerable to produce any certain effect, a piece of apparatus was made by means of which the voltameter could be subjected to a gaseous pressure up to 120 atmospheres. The pressure cage was made by boring a hole 3¼ inches in diameter in a piece of steel shafting, threading a portion of it, and then screwing it down upon a suitably prepared cast-iron base up through the bottom of which the conducting wires passed and the compressing gas was admitted.

The Rayleigh form of silver voltameters was employed. The diameter of each platinum dish, or bowl, was 7 cm., and the di-

mensions of the silver anodes, when new, were 4 cms. diameter by 2 mm. thick.

The insulation of the voltameter in the cage seemed to be perfect, but, to make sure of no error due to leakage, the position of the two voltameters with respect to the battery was reversed in alternate experiments.

The pressure was secured by admitting compressed air from a cylinder into the cage.

The following results were obtained :

No. Experiment,	1	2	3
Pressure in atmospheres,	103	95	103
Deposit in air,	1.1371	1.23745	1.08205
" under pressure,	1.1370	1.23740	1.08190
Difference in grams,	0.0001	0.00005	0.00015

The time of deposit in each case was about 32 minutes. According to these results, no pressure effect seems to have been produced, as the differences, though all in the same direction, are probably within the limit of experimental error.

To see if the air absorbed under pressure exercised any influence, two deposits were made after the voltameter had stood, in each case, during several hours under a pressure of 90–100 atmospheres. The differences in the deposits as obtained in air at ordinary pressure and under the hood, were 0.00000 and 0.00010 grams, respectively.

But it has been observed[1] that gases dissolved in the silver electrolyte affect the amount of the deposit. The result here reported may, however, be reconciled with these observations if it is remembered that oxygen and carbon dioxide decrease and nitrogen increases the deposit. Using air N, therefore, neutralizes the effect of O and CO_2.

EFFECT OF TEMPERATURE.

In testing for a temperature effect, experience suggested a form of voltameter much more convenient for the purpose than the Rayleigh form. The electrolyte was contained in a glass beaker

[1] Schuster and Crossley, Proc. Roy. Soc., 40, p. 344, 1892, also Meyers, Wied. Ann., 55, p. 288, 1895.

on the bottom of which rested the silver anode, while the disk-form kathode was suspended horizontally near the top of the electrolyte, just as the anode ordinarily is. The kathode was made of platinum wire 0.6 mm. in diameter, wound about cross-arms like the bottom of a basket into a close, flat spiral with the suspension wire coming out from the center perpendicular to the plane of the spiral. (If the electrolyte is a water solution a disk of sheet platinum makes a good kathode.) The current was led down to the anode by a plantinum wire passing through a small glass tube. In this form of voltameter no filter paper wrapping for the anode is necessary, and, for small currents, it is a cheap and satisfactory form in practice.

Here again the two voltameters were alike and were placed in series. The silver nitrate solution in one was boiled and then cooled in the same beaker to the temperature of melting ice, and the deposit from this compared with that obtained from a solution maintained at about 90°C. The object of boiling one solution and then cooling was so that both solutions should have nearly the same degree of freedom from dissolved gases.

In these experiments the value of the current was 0.25–0.3 ampère, and the area of each kathode about 13 sq. cm. The weights of deposits from five consecutive tests follow:

From hot solution,	0.79835	0.8434	0.82655	0.86470	0.85900
From cold solution,	0.79820	0.8435	0.82655	0.86420	0.85900
Difference,	0.00015	—0.0001		0.00050	

The average difference in these deposits is again within the limits of experimental error. But this result is apparently not in agreement with that of four observations made by Rayleigh, who found a difference of about 1 part in 2,500 in favor of the deposit from a solution at 50° C. as compared with that obtained from a solution at 4° C.; but it is in agreement with the result of two observations made by Kahle.[1] But if the deposit from a hot solution, or from a solution previously boiled and then cooled, was compared with that obtained from a cold, unboiled solution, the differences were irregular, the former being greater than the latter in amounts varying

[1] Zeitschr. für Instr., 8, p. 229, 1898.

from 0.00–0.08 per cent. This was probably due to the expulsion of oxygen and $C\Theta_2$ from the hot solution.

It is possible that there might be a slight increase in deposit at higher temperatures which was not revealed, owing to some of the deposited silver having been dissolved by the hot solution. Kahle showed that hot water would slightly dissolve the deposited silver. And this was verified by the writer who found that the kathode with its coating of silver lost 0.2 mg. by being suspended during two hours in water maintained at 90° C. Now, it may be that at the instant of deposition the silver is more soluble than after it has been washed and dried. However, an increase of temperature probably does not of itself increase or diminish the deposit, as will more fully appear below.

Though the deposits from cold and hot solutions, respectively, did not differ in amount, they did differ in physical appearance. Those from cold solutions were always fine grained and snowy white, while those from hot solutions were more coarsely granular with a tendency towards gray in color.

In a further search for a temperature effect, the deposit from a boiled and then cooled water solution was compared with that obtained from fused silver nitrate. The results of the last seven experiments are here given:

Temperature of $AgNO_3$	260°	255°	250°	245°	243°	242°	243°
Deposit from solution,	0.7224	.7307	.90250	.79955	.8511	.88965	.9068
Deposit from fusion,	0.7230	.73075	.90285	.79955	.85115	.89080	.90665
Difference,	0.0006	.00005	.00035		.00005	.00115	.00015

Though these results show a difference in favor of the deposit from the fusion, yet this is probably accidental and due to some particles of foreign matter that were not washed off; for there was always more or less of a scum floating on the surface of the fused silver nitrate, some of which adhered to the kathode when removed and was difficult to dislodge completely.

In all the experiments of this paper, the water solutions employed were made by dissolving c. p. silver nitrate in distilled water in the ratio of 15 parts to 85. When fresh, these solutions were all neutral to sensitive litmus paper. After each experiment the solution

was usually poured back into the bottle from which taken through a funnel and filter paper; subsequently it was taken directly from the bottle for use.

Effect of "Used" Solutions.

Two different series of tests were made on the relative deposits from fresh and "used" solutions. The voltameters (Rayleigh form) consisted of two platinum dishes 7 cm. diameter, with round silver discs 4 cm. by 2 mm. The first "used" solution tested was made November 27th, its volume was 300 c.c., and from it about 52 grams of silver had been deposited when this series began, January 16th. Five tests were made and 69 grams of silver had been deposited from the solution when the last test began. The deposits in different tests varied in amount from 1.5 gr. to 2.26 gr. and the current from 0.77 amp. to 1.0 amp. The result of the five closely agreeing measurements gave an average deposit from the old solution greater than that from the fresh by 1 part in 11600.

More than two months later another series of tests on this point was made, the old solution in the meantime having been used a great many times, portions of it repeatedly boiled, and about 55 grams more of silver deposited from it. In this series the deposit from the old solution was compared with that from two different fresh solutions and was greater in amount by about 1 part in 1700. Moreover, this was a percentage increase as was proved by currents of different intensities and deposits of different amounts. But in every case in both these series of tests, the deposit from the fresh solution was more coarsely crystalline (varying from slightly to decidedly) and slightly less white in color than that from the old solution—just the opposite of what Kahle observed. Furthermore, a test at the conclusion of the work with litmus paper and with litmus solution failed to show, with certainty, any acidity in either the fresh or "used" solutions. This again is not in agreement with Kahle's observations. But Prof. Morse, of the Chemical Department of Johns Hopkins University, who was kind enough to make acidity tests for me, gave it as his opinion that the solutions were about neutral.

Possibly the boiling to which the old solution had been repeat-

edly subjected prevented it from becoming acid. This explanation is suggested by the fact that another solution, made some months later, became slightly acid on repeated use without boiling. This latter solution would turn sensitive litmus paper, but still it was not acid enough to prevent the permanent existence of a precipitate in 100 c.c. of the solution, due to the addition of $\frac{1}{10}$ c.c. of tenth normal sodium hydroxide. This solution, therefore, was decidedly less acid than Kahle's, but perhaps it had not been used so much, for only about 42 grams had been deposited from the whole volume amounting to 600 c.c. But the solution was five months old when the acidity tests were made. Age alone, however, did not render it acid for a solution equally old, but unused, was perfectly neutral.

The increase in deposit from "used" solutions must be due to some reduction of the valence of the silver in the electrolyte. Whether this reduction is due to the passage of a current, to light and age, to age alone, or to all combined is a question which it is proposed to submit to the test of experiment.

SIZE OF ANODES.

If, within ordinary limits, the relative size of anodes exercises any measurable influence it is important to establish this fact. It has been observed that larger deposits are often obtained in a large platinum dish than in a smaller one in series with the larger one. Schuster and Crossley expressed the opinion that this is due to the larger anode usually accompanying the larger dish.

To test this matter, the writer employed anodes of various sizes. At first two platinum dishes, each 7 cm. in diameter, were used, and the standard anode was a silver disc 4½ cms. by 3 mm. Two sizes of comparison anodes were tried one 3 cm. by 3 mm., and the other 1.8 cms. by 3 mm. The relative position of the anodes with respect to the battery was kept the same, but the position of the two bowls was alternated in each succeeding experiment. Thus the standard anode was alternately in each bowl, or dish.

Seven consecutive experiments with currents from 0.5–0.6 amp. and deposits of about 1.2 grams each led to the belief that there was no measurable effect due to the relative size of the anodes under the conditions of these tests. An unexpected result, how-

ever, appeared. In only one out of the seven tests were the deposits equal in amount. In the other six the deposit in one dish was larger than that in the other. But one dish was new and had a bright inner surface, whereas the other dish was old, its inner surface dull and scoured. In the six experiments there was deposited in the new dish a total of 7.1956 grams of silver and in the old one 7.19705 grams, which is 1 part in 5000 greater than the other. Now this difference could not be due to any relative error in the weights used to compare the deposits, for, in order to avoid this and obviate the necessity of making tedious corrections, the two dishes, by filing their edges, had previously been made very nearly equal in weight. Thus the bowls and their deposits were weighed with the same masses, respectively. These results seem to show, as Kahle has already observed, that the amount of the deposit is slightly dependent upon the nature of the surface receiving the deposit.

To test further for an anode effect, two platinum bowls, or dishes, each 9 cms. in diameter, the inner surfaces of which had been scoured, were used at kathodes, and the dimensions of the standard anode were 6 cms. by 2 mm. The comparison anode in the beginning was 1.8 cms. by 3 mm., but these dimensions of the small disc grew rapidly less, so that, at the end of the fourth test, the disc had been almost consumed, it weighing less than 1 gram.

The data follow:

No. test.	1	2	3	4
Amps.,	0.5	0.52	1.0	1.0
From large anode,	1.2303	1.0582	1.1592	1.1538
From small anode,	1.2304	1.0582	1.1591	1.1536
Difference,	— 0.0001		0.0001	0.0002

These results seem to show that the relative size of the anodes, except possibly in extreme cases, has no appreciable influence.

Another point, tested by several experiments, was whether or not the filter paper wrapped about the anode in the Rayleigh form of the voltameter has any influence on the deposit. For this purpose a voltameter of the form previously used in the temperature experiments was connected in series with one of the Rayleigh form. J. H. Munktell's Swedish filter paper was used to wrap the anode.

It was found that the deposits were either the same in amount or their difference was within 0.2 mg.

The silver nitrate and anodes used in this work were obtained as c. p. from Eimer and Amend, New York.

The work was mostly done in the Physical Laboratory of the Johns Hopkins University, and the writer is indebted to the Directors of that Laboratory for valuable advice and assistance.

UNIVERSITY OF UTAH, SALT LAKE CITY.

A LECTURE DEMONSTRATION IN WAVE MOTION.

By Will C. Baker.

THE following is a description of a set of simple lecture demonstrations of the main phenomena of wave motion that have proved of great assistance to the writer in the past few years. As no description of them has been found in text-books or magazines, they are communicated in order that they may be of use to others. The experiments cover the same ground as the beautiful work of Vincent[1] and Wood,[2] but are adapted to lecture demonstration. The dimensions given are of small importance, but will serve to give an idea of the scale of the experiments.

A mercury tray—72 × 55 × 7 centimeters—whose smooth bottom is painted white, is filled to a depth of about half a centimeter with water, slightly blackened with ink. The proportion of ink to water will, of course, vary with the ink used, but the bottom of the tray must be clearly visible through the layer of fluid at rest. If now a beaker or other vessel of eight to ten centimeters diameter be placed in the tray a circular wave will be set up that will spread in all directions. The motion of this wave can be clearly followed as, the thickness of the fluid at the crest of the wave being much greater than at the trough, the appearance is that of a dark band travelling over a lighter background. The removal of the beaker gives a second wave; so, if the beaker be kept moving with a period of one or two seconds, a train of waves of period one-half to one second is produced.

It has been found best in lecturing to use either single waves or trains of not more than two or three, to avoid the interference effects after reflection at the sides.

Let a circular wave be set up as described. It travels across the tray at from fifteen to twenty-five centimeters per second and its

[1] Phil. Mag., June, 1897; Feb., 1898; Sept., 1898; Oct., 1899.
[2] Phil. Mag., Aug., 1899.

reflection and the growth of the reflected wave at the sides of the tray can be easily followed on account of its slow motion. If a block of wood, say 50 × 3 centimeters by any thickness, be substituted for the beaker, a sufficiently good representation of a plane wave is obtained. Attempts have been made to eliminate the circular portions set up by the ends, by confining the disturbance between parallel guides, but the gain is so slight as not to warrant the loss in simplicity. With waves of this nature reflection of plane waves may be easily shown.

A semicircular portion of barrel hoop placed at one end of the tray forms a reflector illustrating convex and concave mirrors. A plane wave sent into its concave side converges to a focus, or a circular wave set up in its focus changes after reflection to a plane wave in a manner beautiful to see. Circular waves after reflection proceed to the conjugate focus. These actions are best followed, especially by the class, when single waves are used.

Refraction phenomena are shown as follows : A piece of window glass, say 60 × 12 centimeters, is placed about one millimeter beneath the surface of the fluid and parallel thereto, care being taken that its surface is thoroughly wet. The waves will travel more slowly over this shoal part than they will over the deeper parts of their path. Circular waves in passing over this, decrease in curvature and have a shorter wave-length (for trains of waves) than over the deep part, and in passing off again increase the curvature and lengthen out, as described in the laws of refraction ; the displacement of the apparent center being clearly seen. Plane waves, as described above, at oblique incidences (say 60°) are refracted in passing onto the shoal, in a very marked manner.

A triangular piece of glass, of say 20 or 21 centimeter sides, with stops from its vertices to the sides of the tray to prevent disturbance from other parts of the wave front, enables us to show displacement of the image as with a prism ; the depth of the fluid over the glass regulating the retardation of the wave.

A circular or lenticular piece of glass, immersed at a suitable depth permits us to watch the changes in curvature of the wave front at the various surfaces and its final convergence to a focus as in a lens. As the best results are obtained with a comparatively

small retardation a circular or nearly circular sheet of glass of say 20 centimeters diameter is used.

Huyghen's principle may also be illustrated by placing across the tray a stop having a gap of about one centimeter at its center. Any wave striking on one side of this is reflected except at the gap, which becomes the source of a circular wave proceeding into the smooth water beyond.

The diffraction or bending of waves around obstacles can be easily shown.

To obtain good results the tray bottom must be smooth, of a light color and must be parallel with the water surface. Single waves permit the class to follow each one through its changes, except, of course, where one is dealing with changes of wave-length when trains, must be used.

As waves, and not ripples, are used any lecture table is steady enough for the demonstration.

It may be necessary to draw attention to the fact that in all the foregoing we are dealing with waves passing over surfaces and not through media but the mechanism of the actions are similar, if not the same, in the two cases.

The claim is made that this method permits the class to obtain vivid ideas of the transformation of waves under the conditions dealt with, and that "retardation" and the various other terms applied to wave phenomena tend to take on clear meanings.

PHYSICAL LABORATORY, QUEEN'S UNIVERSITY, KINGSTON, ONT.,
January 15, 1900.

AN APPARATUS FOR THE DETERMINATION OF THE COEFFICIENT OF EXPANSION OF AIR AT CONSTANT PRESSURE.

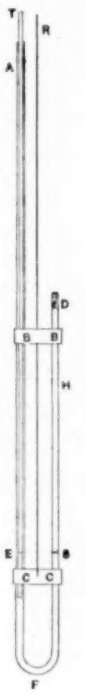
BY C. F. ADAMS.

THE determination of the coefficient of expansion of air is a problem eminently suited to the physical laboratory of the secondary school, but no apparatus that has hitherto been recommended for it is capable of giving accurate results even in expert hands, much less in the hands of a high school pupil. The practice of allowing pupils to use apparatus and methods that at best can give only very rough approximations to the truth is prejudicial to the cause of science and injurious to the pupil; and yet a great deal of the so-called quantitative work in our schools is of such a character. Much of the apparatus that has been prominently recommended for this work is so crude that in the hands of pupils unaided the results obtained are almost meaningless, and such apparatus placed in the hands of the instructor often requires an expenditure of time and effort highly impracticable and after all without any adequate return for the outlay.

The apparatus here described, it is believed, is not open to these objections. It is the result of a long series of experiments by Mr. N. H. Williams, of the Detroit Central High School, with a variety of devices in most of which he attempted to use mercury as a valve to enclose the air, and in which he failed to secure accurate results because of trouble with moisture.

AFH is a glass tube about 80 cm. long and 3 or 4 millimeters internal diameter. It is closed at *D* by a piece of glass rod fused in. The bend of the tube contains sulphuric acid to a depth of about 10 centimeters. *BB* and *CC* are strips of thin brass bent around the two legs of the tube. *CC* can be slid up and down by the wire *R*, while *BB* serves merely as a guide and support for the

wire. T is a glass tube whose external diameter is slightly less than the internal diameter of AFH so that it will slide easily into the tube A but fitting so closely that it will remain at any desired position. This tube T serves as a plunger in the acid by which the surface of the acid at E can be adjusted to the same level as the acid at S.



The apparatus is first placed in a deep cylinder of cold water, a thermometer being supported beside it. By means of the wire R and the tube T the upper edge of CC and the acid at E are adjusted to the level of the acid at S. It is then taken out of the water and the distance from the edge of CC to D is measured by the mirror scale of a Jolly balance. It is then placed in warm water and the adjustments and measurement repeated. The apparatus has been used with temperatures between 10° and 50° C. Care must be taken of course to stir the water so that the temperature at top and bottom of the cylinder shall be the same. The coefficient of expansion is obtained by the formula

$$\alpha = \frac{v'_2 - v'_1}{v'_1 t_2 - v'_2 t_1}.$$

Sulphuric acid is used instead of mercury because with the latter it was found impossible to avoid trouble with moisture. The vapor tension of the acid at the temperatures at which the apparatus has been used is so small that it can be disregarded. The apparatus is extremely simple and inexpensive and such that it can be manipulated successfully by high school pupils. The following are the results obtained by one of our laboratory sections: 0.00342, 0.00360, 0.00368, 0.00357, 0.00368, 0.00370, 0.00376. Another section reported the following results at the close of the laboratory session: 0.00363, 0.00364, 0.00369, 0.00368, 0.00364, 0.00371, 0.00351, 0.00366. Indeed the accuracy of the results is such that it would seem that in the hands of more advanced students the apparatus would give results in which the expansion of the glass could be taken into consideration.

DETROIT CENTRAL HIGH SCHOOL, December 19, 1899.

ON THE CHANGE IN LENGTH OF SOFT IRON IN AN ALTERNATING MAGNETIC FIELD.

BY LOUIS W. AUSTIN.

WHILE doing some work with one of my students on the change of length of iron due to magnetism, it occurred to me that there must be a similar effect in an alternating field, but perhaps modified in amount or character by the more or less rapid alternations. If the change in length requires an appreciable time for its completion, such modifications, it would seem must exist, and the probability of this seemed to me great enough to make it worth while to investigate the subject experimentally.

The instrument used in the investigation is based on an old principle long used in the lecture room to show qualitatively the expansion of solids due to heat. In its original form it consisted of a small roller placed under one end of a bar, which was held fast at the other end. When the bar was heated it expanded the roller rolled and by the movement of a pointer or small mirror attached to it showed to the class the expansion of the bar. In the improved form the horizontal roller is replaced by a vertical hanging fiber or fine wire which is held between vertical plates of plate glass about 2 cm. square, so that it is rolled when one of the plates is moved just as one might roll a thread between the thumb and finger. Below the plates a larger glass fiber of sufficient rigidity is attached to the rolling fiber and this bears a mirror and water damper. A second rolling fiber is hung between the plates at a little distance from the first in order to keep the surfaces apart to prevent friction. In the first form of instrument made and the one used in this work the fixed plate was cemented to a block of wood and the movable plate against which the iron rested was pressed against it by means of a rubber band. With this arrangement I have been able to get results with a glass fiber 0.01 mm. in diameter, but in general fibers

as small as this are extremely difficult to adjust and are very apt to break between glass surfaces. I am now experimenting with other surfaces in the hope of getting better friction contact and less trouble in adjustment. I hope soon to be able to communicate a paper on this work.

Most of the observations recorded in the present paper were taken with a glass fiber about 0.06 mm. in diameter, though for a few of the later ones a fine wire was used. The latter observations being reduced to the scale of the former on the supposition that the maximum elongation with direct current was the same in both cases. Each of the observations given in the tables is the mean of from six to twelve observed throws and the good behavior of the instrument may be judged from the fact that in none of the series used, except in the case of frequencies greater than sixty per sec., did any of the single observations differ by more than two per cent. from the mean. The absolute calibration was made rather roughly by noting the distance the cross hair of the telescope appeared to move on a scale half a meter away when the moving plate was pushed forward by a fine micrometer screw.

The preliminary observations on the change of length in an alternating field were made on a soft iron bar 26 cm. long and 0.7 cm. in diameter, the same one with which I had been working with a direct field. The source of current was the city incandescent lighting circuit having a frequency of 120 per sec. by day and 60 per sec. by night. I found that the curves showing the relation between change of length and strength of field were similar in form for the alternating and direct currents, but that the maximum in the case of the alternating field fell some ten or fifteen per cent. below that produced by direct field, and that this decrease was greater with the frequency 120 than with 60 per sec.

These experiments were rendered difficult by the large heat expansion produced by the Foucault currents and hysteresis which of course made accurate observation somewhat uncertain. I have thus far found no remedy for the latter difficulty, but have rid myself of the former for the most part by replacing the solid iron bar by a bundle of wires of soft Swedish iron firmly bound together and insulated with varnish. The observations given in this paper

were taken with such a bundle consisting of ten wires 27.5 cm. long and 1.4 mm. in diameter. The magnetizing solenoid was 24 cm. long with 12 turns per cm. The dilatometer with the fiber used (about 0.06 mm.) gave a magnification of about 127000 times with the scale 360 cm. distant. This gave a maximum reading of 14.5 cm. on the scale with direct field.

With apparatus kindly placed at my disposal by the department of electrical engineering [1] I was able to work with currents varying in frequency from 10 to 131 per sec. The values of H varied from 7.5 to 375, approximately, no correction being made for the influence of the ends of the solenoid. The larger value was a little more than enough to bring the iron back to its original length. With the stronger fields, especially with the higher frequencies, the heat due to hysteresis rendered the observations somewhat uncertain. I have been able, however, within the limits of field and frequency used, to get very consistent results by taking the readings by breaking the circuit and noting the distance the cross hair appeared to spring back on the scale. This motion is so sudden and so dead-beat that I do not think any larger error is introduced by the heat drift. Thinking that the water damper might influence readings taken in this way, several observations were taken without it, but no change in the throws could be detected.

In Table. I are given the changes in length expressed in ten-mil-

TABLE I.

H	Direct Current.	10 per sec.	30 per sec.	60 per sec.	120 per sec.
7.5	1.7	2.2	2.2		
15.0	9.0	13.2	13.2	14.4	13.2
30.0	29.5	32.4	29.0	25.8	24.5
45.0	38.5	34.5	35.0	31.0	29.4
75.0	41.5	39.8	36.6	34.1	31.0
150.0	30.1	28.0		27.0	
225.0	20.2	18.3			
300.0		8.0			
375.0	−2.8	−4.0?			

[1] My special thanks are due to Mr. M. C. Beebe, instructor in electrical engineering, for his kindness in managing the electrical machinery used.

lionths of the length for direct current and for currents with alternations of 10, 30, 60 and 120 per sec., the strength of field varying from 7.5 to 375 C. G. S. The maximum comes at a value of H of about 75 in all the series, and the general course of the phenomenon is similar in all. With the lower values of H the elongations seem to increase more rapidly with the alternating currents than with the direct. This is perhaps due to the fact that the currents were read with an ammeter which indicated the square root of the mean square of current strength, which is very probably less than the effective strength of the alternating currents in this

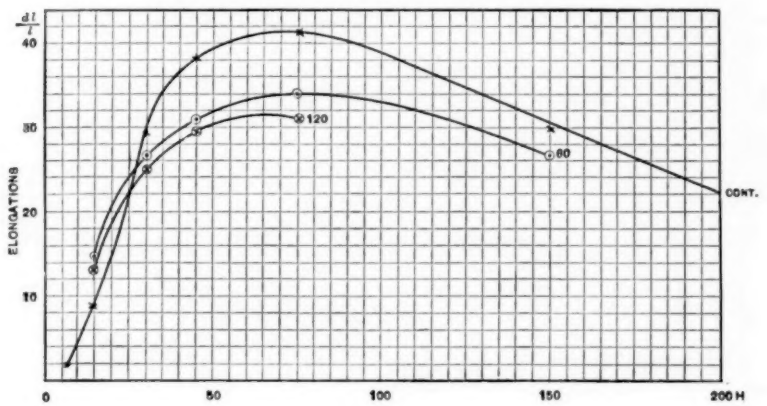


Fig. 1.

phenomenon. With all values of H above 15 the alternating current elongations fall below those with direct current. The curves of Fig. 1 show these relations graphically for direct current and frequencies of 60 and 120.

TABLE II.

Frequencies.	Maxim. expansion.	Frequencies.	Maxim. expansion.
Direct current	41.5	66 per sec.	33.2
10 per sec.	39.8	81	32.2
16	39.6	99	31.5
30	36.6	120	31.0
40	35.2	131	30.7
60	34.0		

Table II. gives the maximum elongation corresponding to different frequencies. A steady drop is noted as the frequencies become higher, which becomes less marked toward the end. Fig. 2, with its rather flat approximately exponential curve, shows the character of the change much better than the table can do.

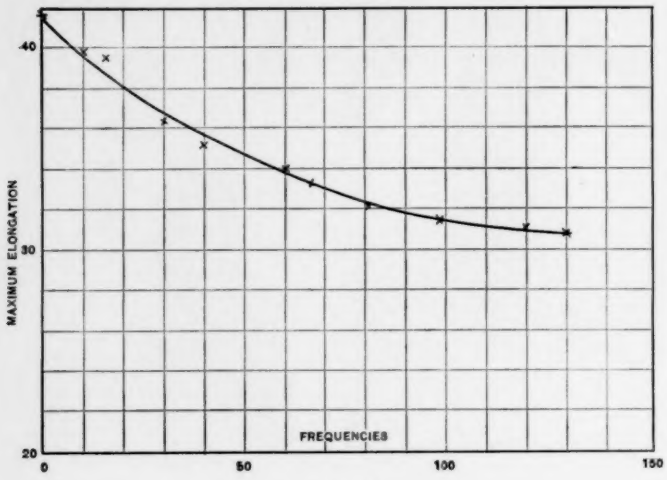


Fig. 2.

An attempt was made at one time to use the secondary currents of a transformer having a Wehnelt interrupter in the primary. The frequency as estimated from the pitch was about 800 per sec. The heating effects were so great, however, that no readings of value could be taken, the scale going by the eye far too rapidly to be read. The maximum certainly fell much below any of the values given in the table and was estimated to be about half that found with the frequency of 131. The results of the investigation may be briefly stated as follows:

1. The general course of the change in length of soft iron in an alternating magnetic field is the same as in the direct field.

2. For all values of H above 25, and at least as high as 375, the length of the soft-iron specimen is less with an alternating than with a direct field.

3. The maximum elongation gradually becomes less as the frequency of the alternations is increased.

This continued decrease of the maximum elongation with increasing frequency is perhaps the most interesting fact brought out. It appears as though there were an appreciable viscosity[1] in the molecular movements depending on the rapidity of the alterations. The last two results may, it seems to me, have a connection with the observations of several physicists[2] on the decrease of permeability of iron when subjected to rapid magnetic reversals. The recent work of Niethammer[3] and of Wien[4] on this subject also shows a correspondence with the third result, Niethammer finding a percentage decrease in permeability with increasing frequency, agreeing fairly closely numerically with the results in this paper. Wien's percentage of decrease is considerably smaller. It can hardly be said, however, that the decrease in the maximum expansion is due merely to decreased induction, since if this were so it would seem that an increase of the field should raise the induction so as to produce the same maximum elongation as before, the only result being to make the maximum correspond to a slightly greater value of H. Since this is not the case the decreased maximum elongation cannot be considered a direct result of the decreased permeability, but their quantitatively similar decrease would indicate that they both may follow from the same molecular causes.

Exactly what course the elongation curves take with higher values of H, I have not been able to discover on account of the heating disturbances. The observations merely indicate that with the alternating current the iron is brought back to its initial length at a lower

[1] Since the completion of this work I have come across the observations of A. M. Mayer (Phil. Mag., 1873, Vol. 46, p. 177) on the time required for the full magnetic expansion to take place. These are the only observations made up to this time, so far as I am aware, that bear on the time factor in this phenomenon. He found that with his strongest current (25 Bunsen cells) $\frac{1}{10}$ second was required for the expansion and $\frac{3}{10}$ second for the corresponding retraction. While these estimates are certainly rough, they indicate the presence of the same appreciable viscosity which shows itself in the alternating field.

[2] Warburg and Hönig, Wied. Ann., Vol. 20, 1883, p. 814.

Tanakadate, Phil. Mag., Vol. 28, 1889, p. 207.

Klemenčič, Sitzungsber. d. k. Gesellsch. d. Wissench. zu Wien, Vol. 103, p. 17, 1894.

Weihe, Wied. Ann., Vol. 61, 1897, p. 578.

[3] Niethammer, Wied. Ann., Vol. 66, 1898, p. 29.

[4] M. Wien, Wied. Ann., Vol. 66, 1898, p. 859.

value of H than with the continuous current. Of the course of the phenomenon beyond this point I can say nothing, but I hope in the near future to overcome the difficulties which have restricted the scope of the work and to investigate the effects both of stronger fields and higher frequencies.

PHYSICAL LABORATORY OF THE UNIVERSITY OF WISCONSIN, January 10, 1900.

NEW BOOKS.

A Text-Book of Physics. By G. A. WENTWORTH and G. A. HILL. Pp. 440+vii. Ginn & Co., Boston. 1898.

The book before us is a disappointment. The authors have won an enviable reputation as writers of mathematical text-books, but in this book they show most convincingly that they are treading upon unfamiliar territory. True, there are many points about the book that deserve praise, and in the hands of a teacher who is a trained physicist the book may be useful; but to those who are only slightly acquainted with the subject, and these are the ones who will use the book most, it will be an unsafe guide. Errors for which it is difficult to find any excuse are unpleasantly frequent. A partial list of these has been prepared by the publishers, and is now sent with the book. Since this review must be brief a complete analysis of the book cannot be given, and the reviewer must content himself with giving a few of the reasons which have led him to the conviction that the book is an undesirable addition to our list of text-books.

On p. 19 we find the venerable fable that "Whatever the nature of the body or the kind of strain, the following simple law holds true: Up to the elastic limit strain varies directly as stress." That this may be branded on the memory of the pupil the same statement is repeated further on and three illustrative problems are given. But the fact is, as most physicists know, that this law of Hooke has been shown to be only a rough approximation.

In the chapter on heat among other minor errors is the statement, twice repeated on p. 102, that "the coefficient of expansion of all gases is equal to .00366" when even elementary books on the subject show that the coefficients for different gases lie all along the way from the value named up to about .00390.

In the discussion of the steam engine we find this statement regarding Carnot's principle: "A denial of its truth would lead to the absurd conclusion that energy can be created out of nothing." It leads to nothing of the sort. The law of the conservation of energy is no more affected by Carnot's principle than is the law of Moses, as was shown by Maxwell a generation ago. The authors state that our best engines "never have a greater efficiency than $\frac{1}{10}$." Ewing begs to differ.

In the treatment of electricity and magnetism the authors follow the beaten track of a generation ago, and the student finishes this portion of the subject without knowing that the medium surrounding a magnet or conductor plays any part. Such an important term as *strength of pole* is erroneously defined on p. 264 and a definition on p. 307 indicates that the international ohm has not yet been heard of. To Faraday alone is given the honor of having discovered induced currents—Henry is not mentioned. On p. 346 energy and power are confounded in the statement "Electric energy is the product of two factors, current strength and electromotive force." The shade of Ruhmkorff would be interested to learn that he and not Fizeau had "improved the induction coil by adding a condenser," but his feeling of pleasant surprise would give place to righteous indignation on being accused further on of having used in it "paraffined mica."

In judging any book one must know its aim. The aim of this book as stated in the preface is "to give a rational explanation of the more important physical phenomena, and to prepare the way for further investigation and study of physical science." If this was the aim at the start it seems to have been forgotten when the last division of the subject, light, was taken up. Reflection and refraction are treated without a word of reference to the wave theory; that is to say, these phenomena are *described*, not explained. Not only are explanations omitted but in many a place the facts are incorrectly stated. Sometimes the errors are typographical, at other times the authors are clearly at fault; for instance, in the article discussing the colors produced by thin films they say "According as this distance is equal to the wave-length or to half the wave length the rays will reinforce or destroy each other." Here they leave out of account the fact that there is at one point a change of phase of half a wave length, and of course, come to a conclusion exactly the reverse of the truth. Some of the optical diagrams are useless and that of the opera glass in particular is misleading and indefensible.

In determining how much space should be given to different subjects it is not to be expected that any two men will agree. Still an author who devotes no less than eight pages of his book to mirrors ought to have found space to mention the diffraction grating. A whole page is devoted to the bicycle while the important subject of thermal electricity has to be content with half a page. Half a page though seems to be more than the authors can write without drawing upon their imaginative faculties. They make the bismuth-antimony couple seven and a half times as powerful as it really is and further say that "for the same difference of temperature, bismuth and antimony give a stronger current than any other combination," a statement that needs some qualification in view of the fact that at least two other couples have much higher electro-

motive forces. In other cases that might be mentioned the authors make statements that are not sufficiently guarded.

One of the truly original ideas of the authors is expressed on p. 157, namely, that the diffusion of two gases illustrates adhesion; but in a text-book accuracy is preferable to originality of this sort. In conclusion the reviewer wishes to express his conviction that any man who writes such a text-book as this, so abounding in sins, both of commission and omission, is deserving of censure. To cast a stumbling block in the path of the young is no light offense.

JOSEPH O. THOMPSON.

AMHERST COLLEGE.

The Elements of Alternating Currents. By W. S. FRANKLIN and R. B. WILLIAMSON. 8vo, pp. 212. New York, The Macmillan Co., 1899.

This book is valuable, not only for what it contains but especially for what it omits, and its authors have succeeded admirably in presenting a clear outline of the essentials of alternating current theory and practice in a concise form, which may serve as a basis for a course of instruction in those subjects. It is so arranged that its treatment of the various divisions of the subject may be conveniently developed by the instructor in one direction or another as the circumstances under which the book is used may require.

Essentially a book for class instruction of electrical engineering students, it will, in an extended course, require amplification by the instructor, as obviously contemplated by the authors, for there has been no attempt to cover in the text many features of more or less importance. The fact that little else than the elements of the subject is included in the work serves to emphasize the fundamental conceptions in a particularly useful manner.

The book has many illustrative problems of a valuable character, together with their solutions, and at the close of most of the chapters are collections of problems for solution by the student which are well calculated to fix in the mind the principles of the chapters to which they are appended.

The first five chapters introduce the reader to the fundamental conceptions of monophase alternating current circuits and to the machines, instruments and measurements required for such circuits.

Chapters VI. and VII. present a satisfactory outline of the use of complex quantities and a number of illustrative problems which may serve as a basis for, and a valuable introduction to those methods which Mr. Steinmetz has so firmly incorporated in modern electrical engineering calculation.

Chapter VIII. introduces the conceptions of polyphase circuits and generators, while chapters IX., X. and XI. present in a more detailed manner the elements involved in the design of alternators and transformers.

The synchronous and induction motors and the rotary converter receive a satisfactory, though brief, treatment in chapters XII., XIII. and XIV., and in chapter XV. is given an exceedingly concise consideration of transmission lines.

The work as a whole is illustrated with well executed and pertinent diagrams, the typography is excellent, and the few errors in the types noted are to be expected in a first edition. The appearance of the book at the present time meets a distinct demand for a systematic outline on which a lecture or recitation course of instruction may proceed to any development required.

HAROLD B. SMITH.

De l'influence de la pression sur les actions chimiques. By GEORGES AIMÉ (1837), 12 pp.; with an introduction by P. DUHEM. 19 pp. Paris, A. Hermann, 1899.

The introduction is entitled *A point in the history of the Sciences. The tension of the dissociation before Sainte-Claire Deville.* Duhem has done a distinct service in bringing about the republication of this remarkable Thesis of Georges Aimé which, as he states, was brought to his notice by M. G. Brunel, Dean of the Faculty of Sciences at Bordeaux.

Duhem gives a biographical sketch of the author and a list of his publications. The remainder of the introduction is an interesting comparison of the work of Georges Aimé with the much later work of Sainte-Claire Deville and of Debray. The perusal, says Duhem, of the thesis of Aimé shows clearly that this physicist had reached the notion of the *tension of dissociation* in the form in which this notion grew out of the experiments of Debray thirty years later.

At the end of the eighteenth century Lavoisier had advanced the idea that pressure opposed the disaggregating force of heat so that at a given temperature a certain pressure would suffice to stop vaporization. Hutton in 1798 applied Lavoisier's idea to the explanation of the geological formation of marble by the melting of limestone without decomposition, the pressure due to superimposed strata being thought of as counteracting the tendency to decomposition. In 1804 the celebrated experiment of James Hall was communicated to the Royal Society of Edinburgh. This experiment was the melting of amorphous calcium carbonate under pressure and the formation of crystalline carbonate. In 1802 Dalton showed that the pressure which equilibrates the vaporizing tendency of a liquid is the pressure of its vapor only, a result which greatly extended the ideas

which Lavoisier had previously advanced concerning the influence of pressure on vaporization.

What Dalton had done in extending the ideas of Lavoisier, Georges Aimé did in extending the theory of Hutton. He undertook to demonstrate that when a substance is decomposed by heat it is not the total pressure which is effective in arresting the decomposition, but only the pressure due to the particular gas which is generated by the decomposition.

Aimé showed in the first place that the decomposition of a carbonate by an acid in a closed vessel is arrested when the carbon dioxide formed reaches a certain pressure which depends upon the nature of the carbonate and of the acid. In the second place he showed that this pressure is independent of the proportions of carbonate and of acid. In the third place he showed that the arrest of the decomposition was determined by the pressure due to carbon dioxide only. He made no explicit statement concerning the dependence of this dissociation pressure upon temperature, although he must have considered the question, inasmuch as he was familiar with Hutton's theory and with Hall's experiment.

These results of Georges Aimé were entirely lost sight of, and it was in 1857 that Sainte-Claire Deville began again to approach the subjects Sainte-Claire Deville's experiments did not completely verify his hy. pothesis of a definite dissociation pressure, which hypothesis he advance- in 1863 when he had reached a clear notion of the matter, and it wad not until the researches of Debray were published in 1867 that this hypothesis became a matter of fact.

W. S. FRANKLIN.

Traité élémentaire de Mechanique chemique fondée sur la Thermodynamique. By P. DUHEM. Vol. IV., 381 pp. Paris, A. Hermann, 1899.

With this fourth volume Duhem's Treatise is completed in about fourteen hundred pages, constituting a most complete outline of the theory of the application of thermodynamics to chemistry. The first, second and third volumes of this Treatise have been previously noticed in the PHYSICAL REVIEW. This treatise represents an immense amount of labor on the part of the author and an exceptional breadth of knowledge of the subject.

It is characteristic of the application of thermodynamics to chemistry that it formulates the subject out of all proportion to our real knowledge, introducing unknown functions and a meaningless plurality of generalized coördinates. This monumental work of Duhem's is, therefore, of less use to the experimentalist than an equally laborious and erudite treatise might be in a subject lending itself less readily to the unbridled play of mathematical generalizations.

In this fourth volume the author discusses what he calls *les Melanges doubles* (double mixtures), meaning systems of two phases of variable composition, such, for example, as alcohol and water liquid and vapor.

W. S. FRANKLIN.

American Telephone Practice. By KEMPSTEAD B. MILLER. New York, American Electrician Co., 1899. Pp. x + 458.

In this volume Mr. Miller offers a wealth of detailed information concerning the systems of telephone construction in the United States. There is but little about the theory of the telephone, and that of the most elementary character, but the description of all the varied parts that go to make up the telephone exchange as it exists at the present day will be found of the greatest interest to all who care for the modern industrial development of electricity. One who is not already familiar with the subject cannot but be struck at the bewildering multiplicity of detail involved in the practical working of a device which when reduced to its essential features is so remarkably free from complicated mechanism as the telephone.

The opening chapters of Mr. Miller's volume lead the technical reader over very familiar ground. The description of the various forms of telephone receivers and transmitters, of batteries and of calling apparatus, together with the properties of circuits, are topics which have been treated in nearly every work on applied electricity which has hitherto appeared. Chapters 14 to 25, however, which form the bulk of the present volume, are entirely devoted to the means by which those who desire to communicate by telephone are brought together. In these chapters simple switchboards for small exchanges, listening and ringing apparatus for switchboards, self-restoring switchboard drops, complete switchboards for small exchanges, lamp signal switchboard, the multiple switchboard, transfer systems, common battery systems, house systems, etc., are treated in a very complete manner with diagrams of the wiring, and excellent illustrations of the more important pieces of apparatus. These follow a series of chapters on the various types of party lines, and the book concludes with a treatise on the selection of wire for telephone use, on the construction of pole lines, and on the laying of underground cables, together with a chapter on line and cable testing.

E. L. N.